WARFARE IN THE HUMAN BODY

WARFARE IN THE HUMAN BODY

ESSAYS ON METHOD, MALIGNITY, REPAIR AND ALLIED SUBJECTS

BY

MORLEY ROBERTS

WITH AN INTRODUCTION BY

PROFESSOR ARTHUR KEITH

M.D., F.R.C.S., F.R.S., ETC.

LONDON

EVELEIGH NASH COMPANY

LIMITED

First Published in 1920

FOREWORD

BY

PROFESSOR ARTHUR KEITH, F.R.S.

AS I read over the manuscript pages of these essays, by my friend Morley Roberts, there came back to me the memory of a night in London, when I set out from a scientific meeting to guide a provincial colleague to his hotel through streets obliterated by a blinding November fog. The way was familiar to me, yet in the end the stranger from the country proved the better guide, for it was he who ultimately took us straight to our destination. By the mere use of his map-fed imagination my friend had mastered the details of London better than I had done after years of residence. Imagination with him had turned the dry and dusty maps, plans, contour-lines and guide books of a great city he had never seen into a living reality, in which he could find his way with confidence, and even offer help to befogged citizens. In these essays by Mr. Morley Roberts we have a parallel case. He has not lived, toiled, and earned a livelihood in any one of the multitude of quarters into which the bewildering City of Modern Science is sharply divided. Yet by the sheer force of his imagination, one which is at once intimate, intuitive, accurate, and vivid he has absorbed the atmosphere of the place, become familiar with its inhabitants, their ways of thought, and their industries to a degree which is rare among even the most experienced natives of this great city. Had he been merely an intelligent visitor to their strange dwelling-place, one who had wandered through its docklands, its business quarters, its Smithfields, its Covent Gardens, its Mayfairs, its Kensingtons, and its Hampsteads, and reported faithfully in these essays what he had heard and seen, then he would have accomplished a rare feat. But he has done more than this ; he is a serious student who has made frequent journeys to the city of science in search of explanations to the riddles of life, and has brought back suggestions and answers which should obtain the ear of all thinking people, and which deserve the closest scrutiny from men of science. I look on these essays as a contribution to knowledge of an altogether new kind. The man who suggests the most likely path to truth stands next in the hierarchy of greatness to him who actually finds it.

How is it possible, the reader of these essays may well ask, that one who has been known these thirty years past to a wide circle of readers as a writer of fiction, can know anything concerning the secrets of life and of disease with which men of science are not already familiar ? The explanation is not far to seek. As a writer of true fiction it was Mr. Morley Roberts' business to study human nature and human action, and to grasp the conditions, under which millions of individuals might be massed in communities, and yet remain free and happy. In the body of the healthy living animal, where billions of vital units are massed together in ordered harmony, Nature has accomplished this miracle. What was more natural, then, than that an author endowed with a vast share of intellectual curiosity and gifted with the diagnostic acumen of the born physician should seek for a solution of his difficulties in the workshops of men of science? He went to them at first as a student of sociology in search of facts which would help him to understand the laws which should regulate the conduct of human beings living under the ever-changing conditions of our modern civilization. But in time it began to dawn upon him, as he watched the labours of the men who are striving to unveil the mysteries of living matter — physiologists, pathologists, psychologists, embryologists, bacteriologists, biochemists, anthropologists, zoologists, and botanists—that the student of sociology had at least as much to give as to receive. He found that the problems which face the students of that most marvellous of living organized communities—the healthy human body—the problems of disease and of health, of malignancy, immunity, inhibition, heredity, cell division, evolution, growth, repair and old age—had their parallels and analogues in organized human societies. He therefore commenced to ascertain how far the obscure phenomena of biology could be elucidated by applying the explanations which are familiar to students of social phenomena. Thus it comes about that in these essays we have records of a unique kind—records made by a layman after years of hard thinking and close observation which he now places before his professional brethren with a skilled pen, a rare wealth of apt simile, using always the diffident and modest language of the real searcher after truth. No one who loves that search will lay this book down unrewarded.

Men who have grown grey in those quarters of the City of Science, which are devoted to the service of medicine, are accustomed to the visits of strangers of diverse types. They have seen chemists like Pasteur and Lavoisier, and clergymen like Stephen Hales and Priestley, force their ways into their workshops, ultimately revolutionizing their industries. They are also familiar with the newly fledged student of first-aid, who breaks his way through the circle of spectators surrounding a street accident, and brushing aside the skilled surgeon, takes charge of the case. Occasionally, too, they come across those visitors who, letting their imagination rise on untrammelled wing, picture for them a future full of marvels. In Morley Roberts we have a visitor of a new kind—one who compels his imagination when in flight to observe the laws of gravity, time, and space. Nay, so like a native does this visitor carry himself, that for several years there were many besides myself that had no suspicion that Morley Roberts, the erudite writer on medical and allied problems, was the same Morley Roberts who is known in Bohemia as an artist of noted skill with pen and brush. In these essays he has earned for himself the freedom of the City of Realities or Science.

With one last word my privileged task of introducing the reader to these essays is finished. Their author has drawn large drafts on the Bank of Science; I, for one, am willing to endorse his bills.

ARTHUR KEITH.

AUTHOR'S PREFACE

THE questions discussed in this book arose originally as side-issues in the prosecution of studies for a much larger book, to be entitled *Social Physiology and Pathology*, in which I meant to deal with the health and diseases of social organisms, as well as with the laws underlying political energy as it seeks blindly to adapt societies to a changing environment. I had mapped out a work, more, I own, than was sufficient for a lifetime, in which inquiry was to be made in the order and failure of order in societies, their well-being and their disorders, and finally discovered that such labour demanded a considerable knowledge of several sciences, especially that of pathology, so that true distinctions could be drawn between fatally morbid processes and those morbid states which foreshadowed, and indeed foretold, new social variations. In spite of the unhappy fact that statesmen and politicians of all kinds ignore science, it seemed to me that such a book might at last prove useful even to them if its trend were understood, and its doctrines appreciated, by a few critics. Yet, finding that those who undertake unprofessional work must at least subscribe to the first of the monastic vows, I put the project aside with regret, although I believe that much of this short volume will indicate to those interested in the complex phenomena of sociology many conclusions as to social diagnosis which only extended labour could make quite clear. Although I cannot carry out the labour originally proposed, such investigations as I have been able to make may throw a useful light on the fundamental principles of social adaptation, and also discover and illuminate a number of vexed questions in biology. It would be ungrateful of me if I did not acknowledge that the impulse to attempt such a task sprang, not, as might possibly be imagined, from the work of Herbert Spencer, but from a little-known book by a great physician, who never received his full meed of appreciation as a teacher. I refer to the late Dr. Henry Gawen Sutton, once a colleague of Sir Andrew Clark's at the London Hospital, whose *Lectures on Pathology*, taken down by an ardent student, contain a sounder criticism of life and more real wisdom than a library of metaphysical treatises. His pathology may, indeed, be out of date, but the knowledge that has passed him by has not yet reached the height of his vision, since his intuition, though sometimes curiously and roughly phrased, often condensed into five words the lifetime's thinking of a true philosopher.

It is generally supposed that any one who is outside the circle of professional investigation, and attempts to enter it with no credentials, does so at the peril of entire neglect if not of contumely. On this point I can only say, and I do so with gratitude, that the encouragement received from most of those best qualified to speak upon the subjects treated has been most generous, while the exceptions to the general rule are so few that the very fact of their existence accentuates the kindly and helpful attitude of the great majority. Even in the cases where I have ventured to differ from high authorities on obscure points which are still unsettled, I have found them ready to listen and eager to discover the possible value of any suggestion.

To subjects purely scientific I have thought it worth while to add a short paper, originally published in *Folk-lore*, which deals with the Thargelian *Pharmakos*. The etymology and significance of the word Pharmakos, and its relations to magic medicine, are very obscure, and any possible elucidation of its meaning should be of interest to such modern descendants of the ancient therapeutic magicians as practise medicine with more modesty as well as with more success. I cannot refrain from stating here that the friend mentioned in the paper, to whom I owed the knowledge of the existence of the Turkic word *vourmak*, was the late Mr. Max Montesole, whose vast stores of linguistic learning were always open to those who could not aspire to equal his own, and whose death no one who knew him will cease to deplore. With this acknowledgment of gratitude I wish to combine my sincerest thanks for help and encouragement to such men of science, who are happily still at work, as Professor W. M. Bayliss, Professor E. W. MacBride, Professor J. T. Cunningham, Professor Marcus Hartog, Dr. Chalmers Mitchell, and Professor Benjamin Moore, while for help given me upon special points I desire to add to these the names of Sir John Bland-Sutton, Dr. Lambert Lack, and Mr. Sampson Handley. In saying so much I by no means imply that what I ventured to put forward always met with acceptance. On the contrary, as may easily be imagined, it often encountered severe, if kindly, criticism, from which I derived the greatest benefit, even though it did not in some cases wholly convince me. Last of all, but assuredly not least, I must thank Professor Arthur Keith, whose almost unequalled general knowledge has ever been at the service of all interested in science, while his openness of mind and his readiness to consider fresh views, whether orthodox or the reverse, are as well known as they are remarkable and exemplary.

MORLEY ROBERTS.

CONTENTS

WARFARE IN THE HUMAN BODY

CHAPTER I

Method in Science

THE general method of investigation, suggestion, and proof used in this volume was originally adopted as a means of studying social disorders and diseases, for if society is an organism at all, on whatever plane of development, it must be liable to disease, and possess a physiology not remote from that seen at work in other organisms of a lowly type. As the work progressed many side-issues presented themselves, and it was seen that if the notion of developmental diseases in man and other animals had their analogues in society, by which we could learn the nature of, and possible remedies for, social disorders, these should present real analogies with bodily morbid states. Such analogies certainly seemed highly suggestive in the physiology of both kinds of organisms. If, for instance, even the casual study of cerebral physiology and neurology threw a light, however dim and uncertain, upon the nature of politics and the methods by which a national organism is directed during normal or abnormal circumstances; if the nature of the tropisms or instincts of an animal, even of a high type, showed real resemblances to the instincts of a race; if the higher association centres, or "intellect," dealing with doubtful issues over the pyramidal tract, showed a marked likeness to the methods of trial and error necessary in investigating the entirely unknown, it seemed quite probable that obscure phenomena of "volition," instinct, and intellect could also be approached with the light held up to us by social phenomena themselves. On advancing further, it even appeared possible, with such a conception to work by, that organic diseases, especially those of development, might in a measure be elucidated by the careful study of social phenomena presenting somewhat similar errors of growth and failures of order. It may, indeed, be said that this method of working up from sociological phenomena, to those seen in more advanced and orderly sciences, promises even better results than the reverse process. By it we advance from phenomena among which we live and act, and of which we are a part, to those rendered obscure by their very approach to economy of energy and perfection of machinery. Whether we understand society or not, we can at least draw some simple conclusions as to the ways in which it works, and if it is granted, as a temporary hypothesis, that the principles of organization are similar throughout nature, it is obvious investigation may show that the assumption is justified by the light thrown upon subjects with which we are less familiar. It seems certain that sufficient use has not been made of these weapons of research.

To criticize accepted methods is to run counter both to class and individual prejudices. This is true in science as in politics, for to the conservatism which revolts against change, there is added the fear that a new orientation of thought may so discount accepted values as to disturb the position attained by the orthodox. Nevertheless, from time to time there arises the necessity of revising not only accepted doctrines, but also the methods by which they are reached. It seems as if such an hour had now come round, for in many of the sciences the accumulation of facts long ago passed reasonable limits, while those who have an insatiable passion for their collection display little energy in putting them into order. Moreover, they appear to resent, or at the least to deprecate, any such attempt on the part of others. According to many " now " is never the accepted time for a new hypothesis, although true method is the application and adaptation of the whole apparatus of reasoning to any given problem. Little has been done to elucidate it, but it surely implies the use of every weapon of analysis in order to avoid all possible waste of available energy. For any advance in thought implies an intelligent logical use of foresight and surmise, and without them science must become at last a mere rubbish heap. Over-insistence on facts and perpetual discouragement of thinking atrophy the imagination, without which the most diligent seeker after truth must presently perish in the pit he has digged for himself. Such resemble a man who makes bricks, and resents the architect and builder using them. This revolt against acknowledged logical methods has sometimes had its justification, but with the general progress of knowledge the life of a radically unsound hypothesis is usually a very short one. If Herbert Spencer's idea of a tragedy was a hypothesis killed by a fact, such tragedies must grow rater if it is recognized that knowledge is only knowledge, and a fact only a fact, when both agree with what is certain in other sciences, and contradict no general principles.

The evil results of extreme specialism, combined with a refusal to appeal to such principles, can be seen in almost every branch of scientific work. In private a professor of pathology may, and too often does, pour scorn upon the labours of the physiologist, which looks much as if he believed that the right method of teaching shipbuilding was to study wrecks upon the beach. Again, the physiologist, aware though he be of the pathologist's failing, is yet apt to take a similar view as regards biology, while the biologist himself, whose work should necessitate an appreciation of all that appertains to all life, completes the vicious circle by ignoring what has been done by students of disease. So much cannot be denied by those even slightly acquainted with scientific, and especially medical, habits of thought. And yet, in spite of this, there are those who know that science cannot be so divided, and that none, who aspires to more than a hodman's work, is properly equipped without a general knowledge of all the sciences related to his own. This may seem a hard saying; but it proposes no more than should be attainable by those with imagination and intellectual curiosity, and it admits, even presupposes, that he must be as ignorant of special details in such sciences as his fellow-workers must be of his own.

It is by co-ordination of knowledge that advances are made. Yet it is common to sneer at the very word "co-ordination." It may be true that the solitary pioneer, or specialist, not seldom hits upon precious facts. But he more often shares the fate of the ignorant prospector who, by ignoring geology, wastes his labour and dies in a wilderness where no gold can be found. The proper method for any scientific man is to employ all knowledge whatsoever, in order to attain such a degree of insight into the value of others' observations as well as his own as to be able to use and test both. To grasp general conclusions in what we know, and to ignore them in what we are ignorant of, is intellectual anarchism.

It may be said that every one admits that general laws apply in all things, and that to insist on the fact is both otiose and absurd. A belief, however, may produce small results if it is not put into practice. Every one, of course, recognizes that nothing whatever occurs anywhere which can contradict the laws of energetics. And yet vitalism flourishes. Almost all will agree that chemistry is capable of becoming in time an exact science, and while yet inexact, has general laws which some day must be shown to exist, even in the realm of what those, who wish to avoid the connotational pitfalls of the word "mind," may be permitted to call "mentation." But, nevertheless, many are prone to argue that conclusions reached in sociology, for instance, can have no meaning for a physiologist, biologist, chemist, or physicist. Though general laws are in action through all nature, their opinion is that any argument founded upon them is an argument from analogy, and a mere illustration. Resting, as they believe securely, on the absurd dictum that it is dangerous to argue from analogy, they refuse to draw any conclusion, even a tentative one, by its use, being ignorant of the value placed on such reasoning by a logician like John Stuart Mill, and forgetful that analogy is pure, if incomplete, induction. For it can only be built on facts.

The bulk of this book was written, and the suggested methods employed, before I became aware that in one place, at least, Herbert Spencer had suggested a way in which sociological problems might give clues to the elucidation of physiological problems. I may, perhaps, be excused for not having read his paper on "Transcendental Physiology" till lately. Indeed, a great deal of his physiological knowledge seems inadequate even for his day, and perhaps shows signs that the facts, or supposed facts, were found for him by assistants, and never properly considered. Whether this is true or not, he did suggest that we might not only work forward from the economy of the animal to the social organism, but that, in selected cases, analogies drawn from the body politic might sometimes be used to elucidate physiological problems. He says : " Hints may be expected if nothing more. And thus we venture to think that the Inductive Method, usually employed alone by most physiologists, may not only derive important assistance from the Deductive Method, but may further be supplemented by the Sociological Method." He does not seem to have suggested elsewhere that much more than hints were to be looked for, or that the method might be employed not only in physiology, but also in biology and pathology. In no place can I find it said that it might prove of assistance in discovering how general principles worked in any science whatsoever, if each problem were worked backwards and forwards from one science to another.

There is no need to go deeply into the question of analogical reasoning. It is sufficient to point out that, used with caution, it is the most rapidly fruitful form of all ratiocination. Maine called it, "in the study of jurisprudence, the most valuable of instruments," even when he uttered a caution against its premature employment. For any useful analogy must show sufficient points of common likeness between two sets of observed facts to suggest that a general law rules in both. A true analogy is not merely a fanciful likeness, such as may be made out of one point. Although Mill wrote : " There is no analogy, however faint, which may not be of the utmost value in suggesting experiments or observations that may lead to more general conclusions," there is no need to push it to an absurd extreme, as Herbert Spencer himself did, when he endorsed Liebig's comparison of blood corpuscles, as a circulating medium, to money. But from one point of likeness we may proceed to two or three, or as many as we will, until there is complete identity in all essentials. To dismiss an analogy, in which there are many points of resemblance, as pure fancy is unwise, to say the least of it, since it stimulates the imagination in the liveliest way. Most advances in thought are made by the imaginative, who yet hold steadily to the view that the most ingenious hypothesis cannot become theory unless it is in accordance with the greater general laws of the more inclusive sciences, and at last enables us to prophesy about unknown phenomena, and to put into order disconnected facts. Used in this way the discovery of suggestive analogies is the parent of real progress, and it ought not to be necessary to say so. If it were not necessary we should see the method used, and students, young or old, would not be so greatly burdened with mere isolated observations.

If, then, we admit that general laws are everywhere the same in their working, however much obscured by the complexities of the less inclusive sciences, and allow that analogies tried by such laws are a legitimate field for the scientific imagination, we must conclude that observed sequences in one science ought to be discoverable in all others. And if certain sequences are clear in one and obscure in another, while there are still sufficient points of likeness to suggest a like kind of explanation in the obscure set, we may legitimately conclude that we are face to face with the same general laws.

To illustrate such points is not altogether easy, since their comprehension depends entirely upon a fairly adequate apparatus in more sciences than one. This is not common, and the very possession of such knowledge seems to render its owner suspect in the eyes of the exclusive specialist. It is easy for him to show that upon recondite and very special points no one but himself can be sufficiently informed. But when we reflect that on these very points few specialists agree, though they are at one on many general principles, we can afford to discount such criticism. It is illegitimate to call any one a sciolist because he is not conversant with every obscure and debatable point, since the real weakness of the sciolist is eagerness to insist on small points, and to fail in the grasp of great ones. Without demanding of men of science any general philosophical theory, we may still ask them to admit that it is the greatest privilege of any worker, when surveying the whole field of knowledge, to discern in it real likenesses. It has been said that the highest type of intellect is that which discovers likenesses, while the second order is apt to insist upon differences. If this is so it must be admitted that the highest type is not common. To hold the balance between rash generalization and a refusal to generalize at all is not easy, and it may be supposed that the qualities which enable any one to do so can seldom be found in those deeply committed to specialism. Yet the ranging of phenomena under superior headings, which is real "explanation," cannot be achieved unless likenesses are seen where none appear on the surface. The history of science shows that any new discovery or generalization has usually been met with hostility by the best informed on special facts. If the work of Harvey had been dependent for acceptance on the vote of specialists it would assuredly have been rejected And if it is now said that the phenomena seen in any society, considered as a closed system or organism, can throw light upon special points of physiology, pathology, and biology, or even on debatable points in physics, such a statement will not easily meet with assent. But the fact remains that those who reject it still admit that the laws of physics rule everywhere, and that the doctrines of energetics can be seen in whatever place work is being done. To admit so much, and refuse to see that in all phenomena there must be discoverable essential points of likeness, is a contradiction. And to say, even if there are real likenesses which enable us to use such a method as a key to discovery, that the time is not yet come, or the knowledge acquired, for such an organon to be used, is simply an assertion without proof. To go on merely accumulating facts, and we must remember that no "fact" is a fact until it is made part of a whole, is after all labourer's work, and within the power of any one with diligence. We may reflect curiously on the truth that the national neglect of science is again repeated by many men of science themselves when they refuse to recognize the place of fresh thought in their work, or, by reason of their conservatism, place more than reasonable difficulties in the way of those who try to co-ordinate their observations.

The logical method here advocated, if it has its dangers, is of peculiar suggestiveness. To apply observations in a well-known science to one in a state of less order, of which the general laws seem still unknown, requires, it would seem, less skill than the art of selecting certain points in the obscurer study, which show that some general law is at work, and using them to solve problems in the more advanced science. To put this as clearly as possible, it may be said that while sociologists need find no difficulty in applying with success the analogies of bodily disorders, pathologists, without a special logical apparatus, will easily go astray in using social phenomena as a guide to the explanation of disease. But used with care, the method may have great results. We are not confined to applying it to physiology alone, nor need we seek real analogies in nothing else than the social organism. If we get rid of the artificial barriers between all the sciences, we can use biology to explain pathology and pathology to explain biology, provided nothing assumed contradicts chemical or physical generalizations. Such a process of regression may seem as obscure as it will appear unsound to the over-cautious, but it is possible that those who are weary of the prevalent method of seeking to explain the facts of any science within its own boundaries, such as is seen in bacteriologists without any zeal for colloidal chemistry, will, perhaps, be inclined to welcome any extension of Spencer's suggestion. To choose short illustrations is not easy, and the best I know are supplied in the body of this book. In this place I shall endeavour to use the method more as a means of suggestion than a method of proof, and shall apply it briefly to mitosis, budding, the nature of the cell-nucleus, and to other problems of heredity.

When dealing so briefly with the inter-relations of the sciences, it is impossible to do more than make suggestions. Yet, even at length, it would not always be easy to show the pathologist that he should recognize what help physiology may give him. I have been assured by a very great physiologist that his notion of pathology was that it tended to death, and need not be taken into account. He had not considered the possible value of repair in evolution, in spite of the obvious truth that in all branches of life, thought, and mechanical invention, breakdowns lead to new contrivances. The biologist, too, may refuse, and indeed does refuse, to consider the possibility of social phenomena throwing light upon the problems of heredity mentioned above, and the transmission of acquired or altered characteristics. It is now orthodox in biology to adopt a modified Weismannism, and no one would be more disinclined to ask whether, in the evolution of societies and their heredity, we can find anything to support or undermine the germ-plasm theory, than an orthodox believer. Nor would he allow us to ask whether there are sociological phenomena which suggest that altered characteristics can be transmitted. Before answering, or attempting to answer, either question as an example of method, it may be pointed out that, even with the example of J. T. Cunningham's work on hormones before them, most of the unorthodox biologists are almost as neglectful of the help in the elucidation of evolutionary problems given them by physiologists, who have worked on the secretions and catalytic functions of the endocrines, as their orthodox brethren. In speaking thus of the biologist it must not be assumed that he alone is indifferent to other work. The orthodox school of psychologists, directly descended from the introspectional philosopher and the theologian, are equally opposed to the biological school of sociologists. We can, in fact, find no school without such "idols." In attempting what is, perhaps, the vain attempt of their destruction, the most simple example I can choose to illustrate the method advocated is what I believe to be a real analogy between the problems of heredity in biology and sociology. I may therefore be permitted to use a portion of an unfinished paper on "The Possible Mechanism of Transmission," with a view to demonstrating anew the obvious fact that, since general laws do obtain in the universe, their particular application in all cases must have essential points of resemblance.

The function of the endocrine glands, in their relation to general heredity, has been studied far too little. It is true that Keith has explained popularly their probable rôle with regard to racial types, but generally speaking the hasty generalization of Weismann, upheld by many whose record with regard to the dangers of premature hypothesis might, perhaps, have safeguarded them, has been a deadening influence upon biological speculation. And, indeed, even those who have studied environment in the belief that direct adaptation occurs, have been too apt to speak of its influence in general terms, rather than to inquire into the means by which it modifies an organism. There seems to be no biologist who has properly grasped the whole possibilities of catalysts as "tools" or instruments by which the functions associated with protoplasm can be activated, increased and, in certain cases, inhibited, or has laid stress on the way in which all that they "create" can once more give rise to other like but more complex instruments. Instead of regarding protoplasm as modified by the tools it employs, we hear of different kinds of protoplasm. The very expression is an unverified hypothesis, and ignores all that has been done on catalytic action by the physiologists. Such assumptions differ very little from those made by the vitalists who explain life by vitalism, and vitalism by life. But when it is seen that protoplasm may, and actually does, alter in accordance with the non-living organic tools it uses, just as races differ in accordance with their "tools" or catalysts, it seems obvious enough that varying organic phenomena follow each other in accordance with the original catalytic tools employed, which, in due order, are specialized by embryonic or highly adapted glands such as the endocrines. For, as some may be lost, so new ones can be acquired, and some again can be modified in changing internal or external environments. Without the help of chemistry and physiology such a conception could hardly have been reached, though a realistic, not verbal, interpretation of Weismannism might have led to the view that "determinants" were catalytic in nature. Since we now recognize such a morphogenetic character of catalysts and hormones, we need assume no other instruments until it is shown definitely that they do not and cannot satisfy the equation of life. If biologists had not ignored pathology by following Darwin's lead blindly when he assumed, without a shadow of proof, that unfavourable variations must be without effect on evolution, they might have inquired eagerly into the causes of disease, and have found that much of it must inevitably be attributed to factors, or the want of them, originally taken up from the environment. The simplest example is, perhaps, that of iron, and the latest recognized that of accessory food factors, fat or water soluble.

If then the success or failure of morphogenesis is to be attributed to such "tools" employed in a particular energizing field of the environment, it is easy to imagine, and even to prove, that they must go over in the sperm or egg-cell, or be re-acquired from the yolk, or from the parent during gestation. Darwin's pangenes can thus be translated into the language of hereditary morphogenetic catalysts.

Such a statement leads to an inquiry concerning the nucleus of a cell. To what extent do biologists believe that it is alive? They write of the nucleo-plasm as if it were, but all the physical phenomena of mitosis suggest that it is not of the complex molecular structure furnished with reversible catalysts dominating and directing anabolic and catabolic processes which we call "life," but that it is composed of chemical substances, which are determinants of future morphogenesis. Such a view is in keeping with Darwin's pangenes, and does not contradict Hartog's physical mito-kinetic interpretation of the mitotic cell-fields.

If, then, by following the suggestions of chemistry and physiology, rather than by relying purely on limited experiment and the microscope, we finally rid biology of the view that there is true nucleo-plasm, and proceed on the assumption that the nucleus is a varying vacuole, or "tool-shop," and food store-house in which catalysts, or determinants, or activators, are kept, and from which they may be drawn by various physical causes, use is being made of at least three sciences, or four if we include physics, and we seem on a path likely to lead to result. At least we shall not ignore the environment, since it is, and must be, the field from which all morphogenetic materials were originally drawn, however complex they appear when used, combined, and specialized by special organs, themselves the earlier results of similar catalysts working in the embryo.

Yet another science may be used to help towards a possible demonstration. An illustration is not a proof, but, as suggested before, when it contains a larger number of points of likeness it ceases to be a mere illustration. The observed phenomena then seem peculiarly related to each other, and if the illustration deals with familiar phenomena the previously inexplicable problem may have an intense light thrown upon it. It was such considerations, combined with the scientific postulate that all biologic phenomena, on whatever plane of development, follow the same laws, which led me to seek in sociology and social life some real illustrations of budding and mitosis, being convinced that if found, they would be of a similar nature.

It seemed to me that such biologic parallels were to be discovered in the phenomena of colonization, especially in examples of definite emigration parties in different vessels. The departure of a portion of the community carrying its own tools and weapons is obviously a real illustration of budding, and indicates far more than, on a casual view, is seen upon the surface. Provided the new environment is not very different from the old one, the new civilization will follow closely on that of the old. But when the environment changes, as new materials are discovered, the form will and must change. A colonial "bud" which discovers iron and makes weapons and tools from it will, in accordance with its environment, and the stresses of life, become either an agricultural, a hunting, or a fighting people. If tools are catalysts, and catalysts are tools, we are surely in possession of some hint as to the mechanism of the transmission of altered and acquired characteristics, and all the allied sciences have lent their aid to the conception.

It is, however, in another illustration that we can best discern the meaning and mechanism of mitosis. What can be a better illustration of an extruded zygote than a ship carrying a party of males and females, and furnished with all the tools familiar to the mother-country in order to cope with what is expected to furnish a suitable, and fairly like, environment? In such a vessel we have the human protoplasm (on its plane probably no more complex than a "unit of life"), and a definite provision of tools and weapons for carrying on the communal life of the new unit. On arrival at its destination we observe at once the influence of the environment. It may be deadly, the "cell" may die, it may be wrecked, or it may never proceed to further development and division. But supposing the environment is good, the new community, with the help of its tools, will repeat in all essentials the life-history of the parent. If, on the other hand, it is not so like the mother-country in agricultural prospects, but more fruitful in game, we should get a hunting community. The iron ploughs would be turned into spears. Furthermore, we must remark that the nature of the new organism depends very largely on what was in the ship, and its nature. If unfertilized, *i.e.* without men, it must either die or get fertilized by native males. In that case the "tools," or catalysts of both parties would be utilized according to the common ability of both. There would arise a different species, and a further budding or colonization would carry away a new set of morphogenetic materials.

Provided, however, that the ship was "fertilized," an iron or wooden ship might develop two different kinds of civilization, especially if they were wrecked, and the tools of mitotic material lost. But without disaster a state would develop in accordance with its tools and seeds and weapons. There is no need to be led away from the physical side of the problem to that complex of physics and bio-chemistry which we call psychology. The knowledge and traditions of the colonists *are* its protoplasmic character, which again has been determined, and *is*, the result of long ages of other tools. That is to say, the character is the tools used plus the protoplasmic energy.

On this analysis we see how transmission of unaltered characters takes place, and even the Wiesmannists may agree. But, furthermore, we observe that race characteristics and habits and customs are modified by the environment, and that a new metal, a new cereal or root or fruit, may not only bring about modifications, but be transmitted. Some real thing, tool or catalyst, is transmitted and carried away when further budding or mitosis occurs. Without pushing these examples to extremes, it is worth showing that phenomena, strictly and curiously analogous with mitosis may occur. If a new colony gets too big for its environment, and is "determined" (that is, driven by circumstances to enforced behaviour) to divide, what then happens? Stock is taken of the weapons, the tools, the food. It is conceivable that all the tools must be assembled and divided. We should in such a case get something like a *mitotic pattern.* It would be rude and rough compared with patterns in cells, just as cell patterns are probably rude and rough compared with an experimental electrostatic pattern of mito-kinesis, where pure physical phenomena are seen undisturbed. And yet it would be pattern in so far as it was a new special order. Some celestial observer, with a powerful microscope, would see peculiar phenomena of arrangement and division, not to be understood or even guessed at until actual division occurred. The human "plasm" would divide: the "nuclear" matter would be parted, and there would presently be two organisms where there had previously been but one.

And once more the environment would play its part. Some new discovery might make a new race. After generations it is conceivable that such a race, furnished with all sorts of acquired means and methods, might find its ancestors as barbarian as we find many primitive races and, in its turn, would send forth colonies to acquire further characteristics, or to lose those which it possessed, and revert to the savage or embryonic state.

If, as Mill declared, an analogy is an incomplete induction, its incompleteness can be compensated for by the discovery of other analogies, so that in the end we approach, and may practically reach, complete induction. I am little concerned with the technical logic of this or any other argument, since school logic is but the skeleton of living reasoning. Live reasoning is the art of persuasion, and usually consists in the choice of examples which reinforce each other so that a contradictory conclusion seems improbable. The view taken as to the functions in development of catalysts, or tools, organic or inorganic, can obviously be supported by the reverse phenomena of involution, disease, and death. If growth depends on the embryonic possession, and later differentiation or acquirement of catalysts, old age and decay as obviously depend on an increasing failure to manufacture, acquire, or use them. According to Child, if I interpret him rightly, degeneration commences at birth, or even earlier. As shown by the decreasing heart-rate, there is a gradual slackening of metabolism possibly due to the strain on the organism of manufacturing its own complex catalysts, and dealing with its own food. Little by little the strain increases, until the organism shows signs of failure, and there is a loss of catalytic balance with concomitant loss of activity. We have to account for the diversion of available energy, and to say no more than that it fails naturally is no explanation. Opotherapy, or the exhibition of activating or inhibiting drugs, may prolong the drama; but the end comes when the body can no longer be spurred on by what it makes or ingests.

There is also in the phenomena of a serious or fatal disease an inverted parallelism to those of growth and life. Infection, or "shock," whatever that may be, affects the functions of every tissue and gland, and many classic cases of "fever" may be mapped out by symptoms caused, not by the infection, but by prematurely vitiated secretions, and the consequent loss of catalytic power to deal with the disease, with nutriment itself, or with excretions. It seems, then, that in birth, life, disease, and death itself, what we witness is the use, acquisition, or failure and gradual loss of "acquirements."

When we consider that in all inductive arguments whatever we can only attain a high degree of probability, a proposition put very clearly by Jevons, it assuredly seems that catalysts can be acquired, as they can even more certainly be lost. Such a theory is not only of value in biology and the ordinary course of practical medicine, but may probably be employed with advantage in the study of the origins of disease lately commenced at St. Andrews by Mackenzie. His research will undoubtedly deal with the future effect on the youthful organism of the passing ailments of children, the lasting results of early innutrition or want of food factors, and into the probability of such disorders as periodontitis having early undiscovered stages which affect the whole metabolic or catabolic machinery of the patient. It should not be forgotten that studies of this kind were suggested by Galton. If considered analogical reasoning should thus tend to support the intuition and clinical knowledge of the physician it cannot be disdained. It is obvious, and should need no proof, that the imagination, controlled by knowledge, is an integral part of the logic of discovery. It is the Mount Pisgah of science.

Since I hope to have shown with some plausibility that such obscure phenomena as mitosis and the vexed question of transmission of acquired and altered characteristics can be illustrated, and made clearer by the examples just given, and since such considerations threw light on the nature of a cell-nucleus, and enabled us to think of it as a store-house, while we look on catalysts as tools picked up on the path of evolution to enable protoplasm to do better and quicker work, it seems at last that such problems as variations of all kinds, healthy or morbid, may really find solutions in the study of sociology as a mixed biological, physiological, and pathological science. And since all developmental diseases are truly variations from the average or normal type, it looks as if in the future such a study might enable the pathologist to discern the real nature of malignancy, and all the disorders connected with the endocrine organs, which are the regulators of development and orderly growth. No study of any science coming finally under the inclusive head of biology can leave us in doubt of the entire interdependence of all parts of an organism, however much such interdependence is masked during normal or static conditions. But when there is a grave state of disorder these relations become obvious. It is so in a " body," and it is so in a state.

During the late condition of Europe such phenomena were to be seen very clearly. Variation after variation followed on stress, and as the nations responded to the strain put upon them, it was seen how energy was diverted from its normal channels and poured, regardless of economic considerations, into new and enlarged growths of offensive organs. There is no need to labour these points. It must have been obvious to every one that we were then (as we are now) in the presence of biological factors dealing with variation, and likely to present, if kept in unrestrained action, all the phenomena of developmental disease. For the essence of all development is symbiotic equilibrium, balance, and symmetry. Without, in this place, applying biology any further to the study of the social organism, it may be asked whether such phenomena do not enable us to grasp, if not in detail, at least in their broad outlines, the nature of the bodily disorders we know as developmental. For all such disorders are either failures of growth or overgrowth, and at the back of them is the hierarchy of the glandular system, each member of which is like a State department claiming so much energy, money, and men, as a contribution towards the active production of the necessary organs, or tools, by which a nation meets the stresses of the environment by increased growth on one line or new growth on another. The functions of stress, failure, and repair, which are as relevant to societies as to animals, are considered in another part of this book.

Thinking upon such lines, and bearing in mind the fact that during abnormal stress there is a tendency to rejuvenescence, marked by the jettison of old ideas, old men, and even of the most sacred customs, since by such a jettison the activity of a cell's or State's protoplasm is thereby increased—just as it is hindered by the reverse process—we reach the conception that such a process can be overdone, and a state of protoplasmic activity attained which is embryonic, or anarchic. No observer of war phenomena can have failed to observe the tendency to weakness in central control, accompanied, and indeed measured, as it was by the increased and violent activity of various departments of State responding according to the nature of the stresses laid upon them. If central (or shall I say glandular ?) control by inhibition had broken down, we should have seen phenomena on a parallel with those of malignant tissues. Perpetual stimulation or irritation by itself tends to overgrowth of the bodily or social tissue or organ involved ; but when such a tendency is not controlled by other tissues or organs, there is a tendency to invasiveness or destructive parasitism. Such observations seem to show that carcinomas and allied phenomena have their analogues in a social body, and preclude us from thinking of them by themselves, or attributing them to special, rather than to general, causes. It is, moreover, impossible not to notice the connection beween such phenomena and the doctrines of energetics. When only so much free energy is to be shared among co-partners, its over-consumption by one implies not only over-activity in one place, but starvation in another, with a resulting loss of balance.

Such particular consideration of certain unsolved problems leads directly to an analysis of any organism as a whole, and as interdependent parts. When taken as a whole, it can once again be regarded as part of a wider, more inclusive organism; but to regard it on any plane as composed of parts, forces upon us the fact that they exist in symbiosis as separate cell states. Symbiosis is, however, usually construed as mutual help, and this is only a partial statement of the facts, unless the organism is static in static circumstances, that is, unless it is perfectly "adapted" to an unchanging environment. Such a condition is ideal "anarchism," a state of affairs in which each unit functions freely according to its nature, and in no way interferes with other units, since it lacks any qualities, or secretions, which exercise irritating or depressing functions on its neighbours. Such a form of life, while ideally possible, has probably never been attained, and ordinary symbiotic equilibrium is only reached practically in organisms of which the parts are not only helpful among each other, but are actually "hostile" in other ways. The use of the word "hostile" is, of course, no more than verbal shorthand to express the fact that each part has its own work, and in many cases its own frontiers, or limiting membrane. This is a rough statement of the biological conception of any organism, and we can only conceive equilibrium in such a symbiotic community as a result reached by, and in spite of, internal stresses. In such an organism each part is excited or inhibited, or both, by the secretions of the other parts. Any secretion is an excretion, but these excretions have found their uses, either as activators or catalysts, or as direct depressants or inhibitors. That the biological conception is universally true of all organisms is suggested not only in biology proper, but by the hostility, open or subdued, which characterizes classes in society, and it suggests, and in many cases supplies, a real key to the comprehension of developmental disease. As was suggested above, it throws a light upon the effects of diseases other than developmental, since death frequently occurs from an indirect effect on parts of the organism, some of which are destroyed, and others stimulated. When recovery occurs, it often happens that the grave disturbance of a violent infection is found to have disturbed the symbiotic life of the organism, and by reducing some part, or gland, to partial impotence, either by excitation or inhibition, leads to later failures of development or to lethal overgrowth. We can thus imagine a slight organic "social" disturbance in a human being leading directly to acromegaly or other disorders of the pituitary, or to myxœdema, Graves' Disease, and all the possible effects of hypo- or hyper-thyroidism.

In carrying the analogical method so far, I am well aware that it will be said that such suggestions are without foundation, that they are true but unimportant, or that they are important and that every one knew them long ago. But I have been more impressed by a single fact than I shall be by all such criticisms combined. When I suggested to an eminent pathologist that, without a considerable knowledge of biology, very much of pathology could not be properly understood or explained, he replied, possibly not without humour, that he had no time for the course of study I was so good as to map out for him. I accepted the rebuke in good part, but could not help thinking what a lamentable thing it was for scientific discovery that each worker in any particular branch of research apparently hastened to forget the very nature of explanation, which is the introduction of order in all forms of knowledge, and the arrangement of every fact under the headings of more inclusive sciences, so that each phenomenon can be seen from all possible points of view.

It was before remarked that there is much well-founded complaint of the neglect of science in England; but the truth is that none neglect it like many scientific men who might be supposed to know better. It is not only so in the sciences directly connected with medicine, but in all others. In no psychology whatsoever can any recognition of the valuable work done by Robertson Smith, Tylor, and Fraser be observed. While the introspective philosopher digs in the morasses of his own mind, and with each shovelful proclaims some individual accident or hasty explanation as a universal truth, the more advanced experimentalist in mentation relies mainly upon the compilation of statistics. But both alike ignore the light thrown upon the workings of the brains of our far-off ancestors, as seen in thought crystallizing into custom, myth, and ritual. The very logicians, who *ex hypothesi* are exponents of reason, prefer, so it seems, to dally in the ruined schools of mediæval philosophy, rather than study the natural logic of the mind of man as shown in every branch of folk-lore. There has been little endeavour, or none that has met with favour, to analyse the natural hostility of group to group, such as is seen exemplified in "the tribal spirit," into its constituents, nor has it been recognized that in every organism, or even manufactured mechanism, the facts of hostile symbiosis are fundamental. Only thus can we link the very passions of politics and all strife to proved law. In another place I have endeavoured, as far as possible, to use anthropology as a key not only to unlock past history, and to elucidate possible factors of human progress, but to show that certain conditions were the true parents of all the enlarged animal instincts and powers of inference seen in the modern human brain. It is easy to fail, but it is a duty to try, and while endeavouring to map out the ancient paths of evolution, we must surely avail ourselves of every scientific lamp however dim.

If the rough suggestions of this paper carry any weight, and suggest reflections upon method, it will certainly be admitted that students have rarely taken sufficient advantage of the truth, that not only is evolution going on all around them in every phenomenon they observe, but that processes vitally similar to those they seek to explain are this day occurring in the great social organism of which they are a part. Without hope of moving those in whom evolution has done its work, and involution has begun, it may be said that to seek to solve the problems of heredity without taking serious notice of the fact that societies give birth to and bud off from other societies, and to rely mainly on microscopic research when great macroscopic phenomena of the same kind are within arm's-length of the worker, appears almost ridiculous. If Lyell worked on the hypothesis that the observed daily changes in the surface of the earth, though due only to causes that seemed too slight to consider, might account for the world as we see it, and even help to prophesy results in future ages, we may say that in all we observe or experience are keys to the problems which other sciences than geology seek to solve. For it may be repeated, even again, that general laws are indeed general, and that each special case is but that universal clad in its peculiar garment of individual particulars. Necessary as the study of these may be to some special application in life, it is by putting them aside, and by divesting truth of its accidents by the use of the generalizing imagination, that the greatest results can be attained. The very evolution of the brain itself has placed in our hands the mighty powers of surmise and expectation, while experience has given us, when we consider in the broadest spirit all that has been achieved, a guide by which we can hope to direct our steps aright.

REFERENCES.

Child, C. M.—"Senescence and Rejuvenescence."

Cunningham, J. T.—"Heredity of Secondary Sexual Characters in Relation to Hormones," *Proc. Zool. Soc. London*, 1908.

Galton, Sir Francis.—"Enquiries into Human Faculty."

Hartog, Marcus.—"True Mechanism of Mitosis," Leipzig, 1914.

Jevons, W. S.—"Elementary Lessons in Logic," 1880.

Keith, Arthur.—"Differentiation of Mankind into Racial Types," *Address, Brit. Assoc.*, 1919.

Maine, Sir H. G.—"Ancient Law" (Pollock ed.), 1905.

Mill, J. S.—"System of Logic," 1886.

Spencer, Herbert.—"Transcendental Physiology," *Essays*, 1901, vol. i.

CHAPTER II

MALIGNANCY

BEFORE trying to show how a general biological and sociological principle can assist the investigator of malignancy, I may say that it was the form of it known as X-ray cancer which led me to attempt a co-ordination of the many apparently unrelated facts connected with it. To one not unfamiliar with speculation from the time of Durante and Cohnheim, it seemed remarkable that such a new aspect of the problem did not lead the medical profession to discard theories formed before X-rays were known. For, in the welter of conflicting opinions as to the causes of cancer, it was at least certain that here were agents which not only might, but, if sufficiently applied, *must* in the end produce it. It seemed to me then, as it seems now, that when such were discovered all arguments as to the part played by " rests," or irritation, or an acquired bad habit of tissues (Adami), or some unknown infection, protozoal or bacillary, were partly beside the point. Those martyrs to science, the early radiologists, must have died in vain if no one recognizes the high importance of the facts to which their agonies bore witness. That radiologists, so far as I am aware, have not seen the full value of X-ray dermatitis and malignancy in cancer theory is, I can only suppose, due to the immense calls upon their time and the peculiar interest of their daily work. But among them orthodoxy has scarcely had time to rear its head, and those who have seen tissues increase or rarefy almost under their own eyes will probably regard with suspicion a hypothetic unique infection, let us say, which has the remarkable power of causing the proliferation of vigorous invasive tissue. Any theory of malignancy which does not co-ordinate their work with all relevant physiological and pathological facts cannot be a true one. But, by taking their labours into account, and linking them with certain physiological and pathological phenomena, it is, I think, possible to show that a fresh general view may reveal its true nature. If it is then seen that there is no invariable single antecedent to malignancy, it must be admitted that there are many exciting " causes" of which X-rays are but one. If such is the case, it follows logically that it is in the tissues concerned, their nature and relationship, that the true cause must be found. Those who have learnt by bitter experience how to upset, and happily more often to restore, somatic equilibrium, will be most ready to admit this conclusion. Dynamite may be detonated in many ways, but the scientific cause of the explosion is not the man with the match, or the motive which led to its use, though these may be causes in law or psychology, or even the fulminate cap, but its inherent molecular instability. Perhaps the most valuable work done of late is that which shows the means and methods by which an unstable organism is kept in equilibrium, and an explanation of malignancy must take it into account.

It is, however, not common for investigators to work under the influence of general ideas which cannot easily be shown to have strict relevance to their objects. Though to avoid this prevents the concoction of fantastic views, it is certain that too strong a revulsion against theory tends to atrophy the imagination, which is the most powerful weapon of analysis. If our hypotheses and experiments are always closely related to the particular matter in hand, we learn to distrust unduly the tentative inductions we owe to those who do not fear to put forward provisional results which seem to have no immediate bearing on investigation. Thus we do not commonly speak of an organism as a republic of cells, or a federation of organs, and though this seems unprofitable to many, we do so with advantage, since it helps to clarify our ideas on general metabolism. The conception is even more useful when it tends to show that symbiosis is not only found in groups or societies, but in those close cell-systems to which we commonly restrict the term " individual." The more such ideas are studied the more fruitful they become, though progress has not so far advanced but that it is commonly taken for granted that the essence of symbiotic life is mutual or inter-organic help. We ignore the fact that when two individuals, and definite cell-colonies may with advantage be called such, preserve individuality, there is in their relations a certain real, if subdued, hostility. Mutual help, even if indirect, undoubtedly exists, but how easily their relationship may become one of parasite and host all zoologists are aware. There is often a great reluctance to admit that what is true of an organism as commonly conceived, is also true of loosely knit human societies, and that the converse is not mere fancy. But when we observe that this fundamental reserve hostility is in fact self-protection in those political federations which help each member even while they provide against encroachment on the part of others, or of the federal authorities, and then compare such observations with organic life, it may not, to those with scientific imagination, seem far-fetched to declare that the phenomena of zoological and political symbiosis are intimately related, and alike biological. Even in the healthy there is always armed neutrality of tissues, and at any time there may come a breakdown leading to warfare in the human body. Such a conception helps us to see that, whatever the waste in material, the methods by which life is built up, from the apparently simple amœba to the hugest empire, are marvellously economical — so economical, indeed, as to suggest that life could be constructed only in one way.

Whether such views are regarded as commonplaces or extravagances every political student will recognize as true the statement of fundamental inter-state hostility, while every biologist or physiologist knows that balance between opposing forces in the organism is a *sine qua non* of its existence. To such a degree is this carried in health, that every definite organ now appears to rule and to be ruled, to control and to be controlled. The regulators of metabolism are also the regulators of growth, and all alike appear conditioned by the chemical messengers of their environment. This is known to be true of the ductless glands, and as we learn more of their functions we may presently infer that all glands, ductless or not, have several functions, and go on to suspect that every portion of the whole body influences every other part, either for good or evil. What was help may become refusal of aid, and what was due inhibition may exhibit itself as destructive. If we carry these general views with us, and seek for light, not only in the lesser laboratory, but in the great laboratory of life all round us in which ceaseless experiment is carried on, we may presently be able to infer from the theory of hostile symbiosis the real nature of malignancy, and to suggest certain paths of inquiry and experiment with the view to discovering a cure.

However much remains to be learnt of the glandular system, it is known that the tissues respond or fail to respond, and that characteristics are moulded in one way or another, in accordance with the presence or absence, the hypertrophy or atrophy, of these glands. When sex is once determined the genital glands dominate growth ; testes are more frequently correlated with larger, ovaries with lesser, size. Ovariotomy allows undeveloped male homologues greater opportunities ; early castration by preventing differentiation preserves female characteristics. If growth and size are mainly determined by the pituitary and thyroid, emasculation appears to permit the pituitary to exercise a greater influence on the legs, since the eunuch's are longer than normal. Among the unsolved problems of these organs is the phenomenon known as unilateral acromegaly ; but the very fact that it occurs, and that perfect symmetry is rare, shows how remarkably a hormone, or regulators which Gley has named "harmozones," can work or be inhibited. It seems that the tissues are moulded according to the stimulation they receive from secretions of which the chemical constitution may presently be as well known as that of adrenalin, which exercises so powerful an influence on the blood-pressure. A bone may be a function of many variables ; but one is a gland placed beside the brain. It seems probable that the parathyroids influence the growth of nervous tissue, since they control the irregular discharges of motor nerves, and we yet learn that some forms of epilepsy are due to hypo-parathyroidism. Thus not only growth, but much normal behaviour, is ruled by what Bland-Sutton well calls a glandular pantheon. That this is obviously so may reasonably lead to the inference that interacting stimulation and regulation is a function of all tissues, and that this is the method of growth and order in every animal whatsoever. All cases of excessive or defective growth must be classed as the result of stimulation, or the want of it, as surely as we see atrophy follow a failure of function or hypertrophy on its excess. But if this is generally true of all the obviously controlled tissues, it may easily enough be true of those which are regulated we know not how, and, if that be granted for the sake of discussion, it seems possible not only to class all cases of malignancy, but to suggest possible means of combating it by other than surgical means.

That some method should be adopted for clearing up the confusion of theory seems obvious when the battle-ground of the cancer authorities and specialists is surveyed without prejudice. Unless there is definite reason for coming to other conclusions, it is usually safest to work on the principle that earnest and able workers are rarely entirely wrong. That the constitutional view of cancer, held by Paget, though undoubtedly "humoral," and therefore suspect, is still advocated by some is not surprising when it takes the form of "predisposition," if the word is interpreted in the light of modern physiology and heredity. To go no further than to speak of the cancerous diathesis, after the more ancient manner, is however a denial of explanation. The theory of infection may also have something to commend itself, if it is only on the ground that infections may stimulate a latent proclivity, though to declare that malignancy is due to a special pathogenic organism is to ask us to believe that every form of it has its own special bacillus or protozoon, or that a single one can exhibit its potentialities in a thousand shapes, while it is necessary to ignore other very definite phenomena which can with difficulty be brought into line with such views. Moreover, few pathologists will admit that what is seen in cancers has any great likeness to those diseases definitely traced to infection. For such a theory to be complete explanation, it would be necessary to class all inflammatory hyperplasias with malignant overgrowth. It is, of course, impossible to deal with all that has been said in support of the infection theory, which at the moment seems the orthodox view; but, so far as I have yet discovered, no exposition of it can be reconciled with the complete pathology and histology of these disorders.[1] All the evidence alleged to support it can be interpreted as irritation tending to upset metabolic balance, and the conclusions drawn from it are not compatible with X-ray cancer, or with the physiological and pathological phenomena at the base of chorion-epithelioma. Such an explanation will, I feel sure, be found a superfluous luxury, and as such to be dispensed with by the economic philosopher. There are also workers who seem satisfied with the notion that the phenomena in question are due to loss of function in some cells, and increase of function in others. This is no doubt true, but, again, that is the very thing which needs to be explained. We are often told that irritation is the cause of cancer, and the mere statement seems to be considered explanation. This is not the case for, though irritation is often followed by cancer, all that is proved is that in some of the organisms concerned resistance to irritation is weakened, whereas in others it is maintained. Not every clay-pipe smoker, even with syphilis, or every burnt Kangri-user in Kashmir, or every chimney sweep, or pitch or paraffin worker, gets cancer. We wish to know why

[1] See Appendix A. *The Infection Theory of Cancer.*

3

these differences exist, and we shall then be able to class malignancy among other phenomena of normal and abnormal growth. The attribution of malignancy to foods is possibly not without value, if it leads to a diet which is not irritating to the intestinal canal, and the fact that salmon or trout fry, when fed abnormally on hog's liver, may, it seems, suffer from an overgrowth of thyroidal tissue, which later may become malignant, is of importance ; but we are still as far as ever from the knowledge of causes which leads to explanatory classification. It appears that all these views are true as far as they go. If it can be shown that they all point in one direction, we should not be far from the truth.

There are, however, other theories to be taken into account, which appear of greater value, since they are more than re-statement, and seek explanation in the nature and functions of the very tissues which become abnormal. Such endeavours take into consideration not only pathology, but physiology as well. If it is said, by the way, that there is no greater hindrance to scientific advance than the separation of physiology and pathology, few, who are not specialists in either branch of learning, will be found to deny it. The opinions, for they are little more, of Thiersch and Waldeyer, have at any rate the advantage of contact with the physiological side of the problem. Thiersch held that with advancing age the connective tissue ceased to be able to hold the epithelium in check. It was a brilliant guess, but it failed to account for carcinomas in the young, nor does it in any way explain sarcomas. Yet how near the truth it was may possibly be shown later, though, according to Bainbridge, the modern view of the function of epithelium during development is that it determines the character of the connective tissue, and that cancer cells mould or determine connective tissue "to their requirements." This may mean much or nothing, for I confess to having seen few such loose statements. Waldeyer's opinion was more complicated than Thiersch's, and fuller of assumptions. He held that the epithelium was weakened and, being pressed on by the connective tissue, was in parts isolated, and thereby in some inexplicable way liable to transformation into cancer cells. Since this transformation is the problem, we should be no further advanced if insistance on the material of change were not distinctly useful. Durante and Cohnheim, also, seem to have been in favour of the theory that the epithelium and connective tissue directly influenced each other; but Cohnheim was led away by the sequestration or "cell-rest" theory which is due to him. Modern research seems to support his opinion that tumours are frequently to be attributed to such causes; but the malignity of some and the benignity of others is still to be explained. It is absurd to suppose that embryonic "rests" always occupy the sites of tumours started by irritation, and Cohnheim himself excepted certain cases where that seems the immediate cause of malignancy. Ribbert held that such "rests" can be created post-natally, and that epithelium, when cut off from its ordinary physiological control, can proliferate malignantly. Implantation tumours by themselves are sufficient disproof of this view. Adami attributes cancer to an acquired "habit of growth." The cells devote themselves to mitosis. After what I have said I need not add that this is merely re-description. It deals with "how?" not with "why?" Green attributes cancer largely to the influence of the combustion products of coal or peat, with a high percentage of sulphur, as well as to low-lying valleys. While such may be contributory factors to a loss of symbiotic equilibrium, they certainly do not "explain" malignancy. For it cannot be too frequently insisted on that true explanation is the classification of phenomena under some more inclusive law. Observations, however useful they may prove as regards prevention, are not explanation. For instance, if it be true that atrophy of the thyroid is common, or almost invariable, in cancer, we are not much further advanced in explanation, although in certain cases, say those of familial proneness to malignant disease, such an observation might be useful.

The sole general results which I am able to extract from the argumentative confusion of the subject is that epithelium and connective tissue somehow or other possess the capacity of invasive aberrancy under long-continued irritation. This may be no more than a re-statement, but it suggests that the only hope of explanation lies in the discovery of the reasons for tissue stability or instability. Is there any reason for supposing that instability or invasiveness is in certain conditions a physiological quality in epithelium? That connective-tissue cells are capable of reparative work of an invasive order we know already. It seems that the reply to the question about epithelium is ready to hand. Bland-Sutton was, perhaps, on the very verge of a possible explanation of cancer when he declared that in the normal action of the trophoblasts of the fertilized ovum could be seen the physiological type of the invasive action of epithelium. In chorion-epithelioma such a physiological type becomes pathological. This dictum implicitly asserts that where the trophoblastic action becomes malignant, there is a loss of balance; the multi-nuclear cap of the villus is not inhibited by the normal uterine reactions which usually prevent such invasion. What is it in the normal uterus which does inhibit it?

We are not going beyond what is known of repair if we say that the reaction tissues are mainly connective. In the normal gravid uterus the erosive action of the trophoblast is thus in all probability stayed by a connective-tissue reaction. Yet this erosive action *is* malignancy. Cells in contact with the trophoblast dissolve—are, as it were, digested. As the larva of the blow-fly dissolves dead cells, so the trophoblast cap dissolves live uterine cells, by some chemical product, some cytolitic secretion. In normal gestation such action is neutralized sooner or later, and since malignant epithelium, when active, pierces connective tissues as if they did not exist, the reaction which stays its course in the uterine wall must be more than mere fibrous growth. We seem compelled to assume that some cells can neutralize malignant cytolitic action by their products, and thus restore physiological balance. The resumption of pathological action in chorion-epithelioma comes on in the period of involution, when all the uterine tissues lose their activity. It seems hardly too much to say that the secretions or cell-products of the active connective tissue are those which inhibit, or fail to inhibit, the alien epithelium. H. B. Spencer, after quoting Sir John Williams, who stated that pregnancy had no influence in causing benign ovarian tumours to become malignant, and that in old women such are rare, goes on to say that the cause of this rarity cannot at present be stated. A light is, however, thrown upon these facts if it is remembered that during gestation and the retrocedence of the aged ovary there is great connective-tissue activity.

If the implications of the argument are clear, it will be seen that the conclusion to be drawn tentatively is that in such reactions lies hidden the mystery of malignancy. It will, perhaps, be objected that the multi-nuclear cap of the trophoblast is in a sense of alien origin, whereas ordinary malignant growths are autochthonous. In this very fact of partial alien origin lies support of the view suggested. It is more than conceivable that the male element in the zygote is here the earliest possible origin of malignant energy. It would not be wholly surprising if future investigation traced such cases to the peculiar energy of some spermatozoa. The ease with which a sperm-cell enters the unfertilized ovum might be a measure of the likelihood of chorion-epithelioma, provided that the resistance of the ovum were a measure of the general tissue resistance of the maternal organism. But even granting that the alien, or partially alien, origin of the trophoblast renders malignancy more likely than with ordinary somatic tissues, it may be replied, on the lines adopted at the beginning of this paper, that all such tissues are, in spite of their symbiotic life, fundamentally alien and hostile. A breakdown in their relations as established by evolution may, and in many forms of disease does, occur. By the study of the glandular system the interdependence of all tissues is inferred. There is, also, undoubtedly self-protection. "Thus far and no farther" is embryological law. With deficient inhibition we see this law abrogated. For in a new environment we may see any variation. Thus the polymorphism of malignant epithelial cells described by E. H. Kettle is just what might be expected on the loss of normal control. The whole body is a group of organs and tissues which are not always harmonious, and the behaviour of malignant or benign aberrant tissue is by no means a phenomenon standing by itself. Probably all tissues might become malignant if they were as capable of free and rapid proliferation as connective tissue and epithelium. Invasiveness is natural to embryonic tissues. But have not embryologists been apt to regard the cessation of invasion at a given stage as a " natural " fact, *i.e.* just the result to be expected of that kind of cell or tissue? But to cease growing means either a failure of energy or inhibition, and growth must be analysed into excitation and inhibition. In a very true sense " malignancy " or invasiveness is characteristic of all growing tissue. It is not a wild illustration to point out that in society we are all potential criminals at the mercy of excitation and inhibition, nor otiose to observe that the liability to crime on the part of aliens in this or any country is due to unaccustomed stimulation and the lack of former inhibitions. Such criminality is an analogue of malignancy. I owe to Professor Keith the suggestion that the negro in the United States is even a better example. The negro community there is as much a transplanted tissue as a cancer metastasis, it tends to spread, excites violent reactions, and might conceivably prove definitely malignant. I am aware that the remark may excite ridicule; but it can be pointed out that the reaction against the immigrant negro in the north is comparatively slight, and that when trouble occurs it is very frequently due to the presence of a Southerner who, by his previous contact with the race, has been " sensitized " so as to react violently. Without desiring to push the analogy to its farthest extreme, it is obvious that a large negrine irruption tends to break up and push apart previous social bonds and regulations. I do not see how it can be denied that such illustrations help us to understand the more obscure somatic phenomena.

It must be quite obvious by now that the views here advocated link the general theory of malignancy to the doctrine of the endocrine organs, that glandular hierarchy or pantheon which rules growth and metabolism. When we observe that the absence of a particular secretion limits growth, or that its undue increase makes such growth abnormally large, we are assuredly dealing with phenomena closely connected with the existence of epithelial or connective-tissue neoplasms. In both sets of phenomena the root fact is failure of proliferation or its excess. If we delve deeply enough into causes it will not seem absurd to put cancers and giantism or acromegaly into related sub-classes. That the latter are due to abnormal glandular activity we know. With normal pituitary influence no overgrowth occurs. Connective-tissue proliferation ceases at a point when the glandular system becomes balanced. This is obviously the case with what we see in repair of normal epithelium and connective tissue. When the epithelium is stripped away, and the underlying structures damaged, the connective-tissue cells proliferate rapidly. As the young epithelial cells invade the edges of the wound the underlying cells become fibrous and deep scar-tissue. Excessive and unhealthy granulations only arise when the epithelium does not do its work. Histologically there is a great likeness between round-celled sarcoma and granulation tissue, and, after all, granulation is no more than connective-tissue cell proliferation growing outwards into a wound where normal tissues are wanting. A sarcoma might almost be called inverted ingrowing granulations. That the varied phenomena of malignancy exceed in variety those attributable to merely defective or hypertrophied glands is only what might be expected. Such glands are highly specialized epithelium with very definite work. The general epithelium of the body is much less differentiated and nearer the embryonic type. It is found practically everywhere. Connective tissue is the somatic network; in no part is it absent. It exhibits a remarkable capacity for many forms of rapid specialization, and may be looked on as highly unstable because of these very qualities. But its instability is obviously a function of many variables. From the universal presence of these two tissues we infer that normally there is nothing in the organism which inhibits the existence of either in any part. They can grow anywhere, and if aberrancy occurs at all it is in them we should look for it, if they did not, in some analogous to the action of the endocrines, inhibit each other's undue growth. The age incidence of sarcoma and carcinoma suggests most forcibly that they do so. Sarcoma is predominately a disease of youth, though it may be found at any age. It may develop *in utero*. Repair is most active when it is commonest, and epithelium is most delicate. Epithelium reaches a peculiar state of activity, as shown by its products and conduct, at a later age in which connective-tissue activity is lessened and repair slackens. It is a period in which persons of failing metabolism tend to accumulate toxic products in the connective tissue which depress and inhibit its activity. It is the age of cancer. When cancer occurs in the adolescent there is frequently a history of heredity. That the unbalanced should breed unbalanced offspring is not surprising. The cancer house and the cancer valley are unhealthy, most low-lying. From a defective environment we do not expect tissue health or balance, *i.e.* the normal influence of one tissue on another in a federated system. It is by no means necessary that such influences must be exerted by definite glands. Every glandular secretion is but a specialized form of some unknown epithelial product. Snake venom arises in a specialized salivary gland; the secretion of the salivary gland in embryonic epithelial cells. Epithelium as the parent tissue of the true glands must have its own unspecialized secretion, which is poured into the circulation and exerts its influence everywhere. We can hardly go wrong if we say that every cell in the body influences every other cell, and that those which are in an immense majority have much power. Newton's law of gravity might almost be translated into a somatic law, even if some physiological Einstein presently corrected it.

It is, therefore, by no means mere guesswork to assume that the relations of epithelium and connective tissue are the essence of the cancer problem. Quite independent of their cross action in repair we actually see in atrophic "scirrhus" of the mamma that this slowly developing cancer is surrounded by more or less dense strands of fibrous tissue, and is often known as withering or contracting cancer. Patients may live for twenty years or more with this variety of the disease. In old age the connective tissue appears to give way, and the few imprisoned anarchic epithelial cells may resume their invasive qualities. It was such cancers which showed the older physicians that there were attempts at repair in malignancy; but they attributed its arrest to mere mechanical action, a view not tenable when we consider the great erosive effect of really wild epithelium. Handley has shown that in melanotic cancer at a later stage of permeation, there is inflammation accompanied by round-celled infiltration and fibrous growths, while in Paget's Disease there is peri-lymphatic fibrosis. From the experiments *in vitro* of Champy, much can be learnt as to the relations of these tissues. He demonstrated that when renal tissue was grown in a nutritive plasm it showed, after nine hours, new tubules of a primitive order, while still further away from the original section the epithelial cells did not form tubules, and were of a simple embryonic type. This can only be attributed to loss of control by normal inhibitions. When the same worker cultivated simpler epithelium and connective tissue together, the epithelial cells retained their characteristics; but when they spread and grew apart from the connective tissue they lost their usual order and appearance, and were no longer true epithelium. Only one inference can be made. It is that these tissues are to each other controlling environment. Bayliss says in commenting on this, "It seems that cells, when they have taken special functions in the organism, are normally prevented by some means from continuing their primitive multiplication, and that when this influence which restrains their growth is removed, they start afresh and produce simple embryonic tissue. There is significance in these facts in connection with the formation of malignant tissues." Assuredly nothing could be truer and, working with the analogy of the endocrines, we are forced to conclude that like effects are produced by like causes. The "influence" at work must be some product of the connective tissue. In an unstable organism any depressing factor inhibiting the activity of that tissue, such as uneliminated katabolic toxins accumulating in the lymphatics and connective tissue generally, may end in allowing the explosive epithelium to break out into embryonic activity. Such instability has many analogues in pathology.

If it be granted that these facts are of importance, it seems that it is by using them, and by following the indications afforded us by chorion-epithelioma and X-ray cancer that we are likely to solve the problem. No doubt it may seem strange to bracket such diseases, but if it be found that two disorders so different in origin point the same way we cannot be far from the truth. In X-rays we have an exciting cause of epithelial overgrowth which not only may, but if sufficiently applied, must produce malignancy. The symptoms of X-ray dermatitis are those of profound irritation, epithelial overgrowth, attempts at repair, which in mild cases succeed and in severe ones fail disastrously, leaving the skin in epithelial anarchy. It is cracked and fissured in every direction, heaped up in one place and broken down in another, until it becomes a picture of disorder rarely seen even in the domain of dermatology. Such an exhibition of ineffective energy spent at the surface in vain efforts at repair makes it less surprising that what we may call the potential of the deep epithelial layers of the epidermis becomes abnormally kinetic. It may be said that the cells of that layer grow malignant because they find existence impossible in their normal position. It seems certain that in large doses the effect of the rays on connective tissue is depressing: they are, at any rate, totally unable at the last to resist, either by mechanical or chemical means, the push of the escaping epithelium. It is stated by Darier and Wolbarth that in X-ray dermatitis there is hypertrophy of the epidermis, and pronounced degeneration of the corium, the most marked result being, as I anticipated before I was aware of the actual facts, the rarefaction of the subepidermal portion. I may also mention the work of Lazarus-Barlow and his co-workers at the Middlesex Hospital. He points out that in certain conditions the influence of radium rays is one of stimulation. In experiments on rats, which produced what can only be described as squamous-cell cancer, it is especially to be noted that there was degeneration of the subjacent connective tissue, which even extended to bone and cartilage. Obviously radium was here used in time quantities, which carried stimulation into degeneration. These changes are, I may perhaps venture to say, only such as could have been predicted, and I did in fact predict them before being aware of his results. The same can be said of those obtained lately by Russ and his colleagues with regard to lymphocytosis, leucopenia, and immunity. They add immensely to the value of their work by pointing out that a large lymphocyte count is not by itself sufficient to procure or preserve immunity. If the general connective tissue and its catalysts are not active this is to be expected. These catalysts, immune bodies, or anti-bodies, are almost certainly connective-tissue cell products. Russ, indeed, says there is some as yet undetermined relationship between the number of lymphocytes and the occurrence of immunity. But if cancer actually depends on the weakening of connective-tissue cells of all kinds, the relationship is no longer undetermined. We have a real explanation why large doses of X-rays, which are more or less fatal to lymphocytes, destroy immunity, and we get a clue to the reason for small or stimulating doses conferring it on susceptible animals. Hernaman-Johnson states definitely that clinical observation and microscopic research show that carcinoma is favourably influenced as the result of this dual action. Mathematically speaking, the good influence of small doses acts as a "couple," the peccant tissue is inhibited, and the limiting or resisting tissue is stimulated to activity. With such phenomena before us there is no need to posit some unknown cause. In all explanation it is illicit to import the unknown when the known can be made to account for the facts. If radium and X-rays, according to their dosage and application, can cause different effects in both tissues, and by restoring or impairing them produce amelioration or further destruction, the case for infection falls to the ground. It is also said by Knox that the curative effect of radium depends in many cases on the Becquerel rays stimulating the connective tissue and producing fibrosis. Under the battery which brings about these results it can hardly be thought that any specific cancer protozoon continues to live. Those who believe in the infection theory must take up the position that the agent is everywhere in the body or the environment ready to infect the patient in the so-called pre-cancerous stage of X-ray malignancy. Such a hypothesis is a multiplication of causes. The only true pre-cancerous stage is seen when the underlying connective tissue weakens or rarefies. This is assuredly the case in leucoplakia, although I have not so far been able to see sections proving it. The destructive powers of the X-rays on connective tissues, in combination with the resistance of the skin, are sufficient to account for the results. In what other way can we interpret the conclusions reached by J. B. Murphy and Sturm ? These workers found that the entire lymphoid tissue of the body could be destroyed in from seven to twenty-one days by repeated small doses of X-rays. In such a condition the theory of this paper would infer that an immense reduction of organic resistance followed. What do we find ? First, there is a greatly lessened resistance to alien implants ; second, a lowered resistance to cancer grafts ; third, the destruction of acquired immunity to cancer ; and, fourth, a lowered resistance to the tubercle bacillus and other infective agents. Murphy, however, remarks that the chief objection to accepting the lymphatics as a great factor of resistance to cancer growth is that the lymph nodes are common points for metastatic growths. This appears to be no such objection as he imagines since the very existence of the primary focus is in all probability due to general loss of tone of all connective-tissue cells, stationary or wandering, highly evolved or semi-embryonic. I draw the conclusion with confidence that, as with chorion-epithelioma, where infection is negatived by the whole of the phenomena of ovum and uterine interaction, X-ray malignancy and allied phenomena point straight to the conclusion that the explanation of cancer lies in the relations of epithelium and connective tissue; that benignity is a normal reaction and malignity a failure; that irritation is only a means by which the normal reactions of these tissues are destroyed; and that infections are only causes so far as they excite or depress and thereby destroy the balance of tissues which exercise outside control by their mechanical nature and products. I have so far found no theory but the one here advocated that reconciles all these phenomena, and it is a fact that it enabled me to prophesy many observations quite unknown to me at one period of investigation.

Conclusions of this kind are necessarily as relevant to sarcoma as to carcinoma. The immense activity of connective tissue in youth suggests that it might at any age get out of hand. Fowls seem specially liable to it. Luckily they mostly die young. An aged fowl, which should be liable to carcinoma, is a rare object. As a domestic animal which, owing to the caprice of breeders, is in a peculiarly fluent condition, it is particularly liable to loss of balance. Uterine or mammary cancer is rare in bitches, a fact very properly attributed to their commonly dying before involution sets in. It may also be due to their habits, since they are not so much exposed to sexual stimuli as human beings, who only practice continence during the œstrus. It is said that castrated animals are more liable to malignant diseases than others. They have been thrown out of normal balance by operation. The peculiar deadliness of sarcoma seems natural enough if we remember that it is to connective tissue that all repair is due. It is a case of "quis custodiet?" when the guardian tissue becomes anarchic. Whatever influence epithelium may have upon it, epithelial tissue cells cannot surround, or attempt to encapsule, aberrant connective tissue, for as soon as they proliferate freely they are themselves malignant. It seems to me that these views make it easy to understand why a healed gastric or other ulcer may become the originating point of cancer. That there is ever an ulcer at all shows that connective-tissue reactions are weak. When such an ulcer heals there is scar tissue with epithelium already some stages on the way to embryonic epithelium. *Ex hypothesi*, the underlying fibrous tissue is not very resistant, and when the irritation continues which first caused the ulcer the over-stimulated and already partly wild epithelium proliferates, and is not properly inhibited. Given such conditions, carcinoma can be predicted. The results are no longer a puzzle.

That benign tumours should often become malignant is, according to the theory advocated, just what might be expected. With senescence there is in the whole body an increase of static elements as compared with the cytoplasm ; a tendency to rigidity, and a loss of the federal unity of the body which we call health. There is less response to regulative stimulation or inhibition, and less or more of the normal hormones to respond to. The result should naturally be an increase in the autonomy of separated parts, and the increasing dominance of any tissue which is in excess. That the chief tendency of malignancy is towards carcinoma, is what we should expect at an age when epithelium in any case tends to become rampant, but that a benign connective-tissue tumour, in which the epithelial portions are at a minimum, should at last break bounds is by no means surprising. When thinking upon such lines, and dealing with phenomena of senescence, it is a not uninteresting speculation if we venture to attribute to a temporary rejuvenescence the partial cures or alleviations of symptoms often found when a new empirical remedy is tried in inoperable cases. To inspire hope by whatever means is a function of the physician and, to do so is, in the language of the physicists, to free energy. The hopeless patient, when concentred on his symptoms and his feelings, is doubly the host of a parasite, his energy is bound within a narrow circle, his horizon of life contracted to a mere point. As a result his functions fail: he eliminates less and less toxin, the static elements increase till the cytoplasm of his whole organism is as unable to cope with its work as his cerebral cytoplasm is to face the general situation. If he is afforded hope in any way whatsoever the engine works again: there is at least a temporary rejuvenescence, and the partially freed tissues tend to resume their functions. At such a stage the progress of a tumour may be arrested by the renewed action of connective tissue or epithelium, or of the general regulative metabolism of the whole body.

Though cancer "cures" may thus exercise a favourable, if brief, influence on those who suffer, their number and character bear bitter witness to the confusion of the whole subject. In theory I have been unable to find any general principle at work. If it were not that in looking back upon the past of pathology it is seen that most advances have been made rather by trial and error, than by any great grasp of the human mind, those who are not wedded to one particular theory might indeed feel hopeless. Amid the din of battle, the confusion and the shouting, it is hard to discover order. Yet to those who are somewhat withdrawn from the arena, facts do sometimes emerge which seem of real relevance. The long-known occasional cure of cancer due to erysipelas is one of them, and the very failure of Coley's fluid, composed of the toxins of *S. erysipelatosus* and *B. prodigiosus*, to fulfil the hopes of its inventor, may, if considered in a proper light, be of the greatest assistance. That these toxins, without the acute attack, fail of their purpose, suggests very forcibly that it is not such toxins which inhibit the growth of the aberrant tissue, but that it is overcome by the immense reactions of the connective tissue which result in the cure of the acute infection. So far as Coley's fluid excites the connective tissue, so far it may possibly do good. Such a view is greatly strengthened by the experiments of Ehrlich and Apolant, if they can be regarded as authenticated. This is, I think, thought by many not to be the case; but their results fall in so completely with the views advocated in this paper, that I find it impossible to disregard them. That a transplanted mouse carcinoma should in certain cases produce sarcoma seemed to some impossible ; and to some a proof that the transplanted tissue was really sarcomatous. Yet if it is granted for the moment that epithelium and connective tissue live in symbiotic hostility, such a phenomenon is by no means so surprising as it looks. It is but reaction overbalancing itself. On continued transplantation with one strain it is said that the connective tissue overcame the epithelium, till it at last consisted of scattered cells only, so that finally the graft was a pure sarcoma. In another strain this "power to induce sarcoma" was lost, and the tumour remained epithelial in character. The phrase "power to induce sarcoma" is, to say the least of it, unhappy. By the phenomenon, if correctly reported, we have to understand that the host's connective tissue did not react. No explanation of these observations is to be found in any theory but that of the action and reaction of the two tissues concerned. When their balance is upset, one proliferates abnormally. Anything that throws the organism out of gear is a possible factor of malignancy, and that is the reason why, with the increase of wealth, a new and highly varied environment, which tends to produce variation, makes for the increase of such disease.

If the value of a theory depends on the aid it gives in explanation, the one here advocated certainly helps to make it clearer why some forms of malignancy are more deadly and liable to metastasis than others. So far there has been no real explanation of the fact that the forms of it which deviate most widely from the tissue of origin are most rapid and destructive. It has remained an observation, and to say this extreme aberrancy from type renders it more deadly is only to repeat in another form what has been said before. But if it is understood that the immediate and total somatic environment determine cell character, it is obvious that extreme aberrancy implies that the determining tissues generally are weakened to an extreme degree, and that any cancer growth or embolus will nowhere meet with much resistance. That environment has definite results is well known. In speaking of the relatively more deadly femoral sarcoma, as compared with a similar tumour in the tibia, Bland-Sutton says: "This would appear to indicate that the two tumours, though structurally alike, really have different causes, yet these are facts which lead us to suppose that variations in tissue actually constitute a different environment." He adds that echinococcus disease is the only condition which supports this view. Yet surely in studying all diseases we are compelled to come to the conclusion that different reactions, in different patients, with the same disorder, can only be due to their bodies constituting a different environment. Further investigation will almost certainly show that there is some reaction difference in the region of the femur when compared with that of the tibia. The comparative immunity of joints from a burrowing sarcoma supports the view that some tissues have a more powerful resistance than others. It may be that the great resistance of cartilage is due to its lack of channels; but it is far more likely that it is due to the cell products of its closely arranged cells. In studying the various types of malignancy we cannot but be struck by the varying amounts of normal, or fairly normal, connective tissue about them. That the small, round-celled sarcoma should be more deadly than most of the other varieties is what would be expected from the scantiness of the still growing or surviving stroma. Such varieties as are more difficult to distinguish from normal cells seem obviously those in which the whole of the normal inhibition of the environment has not been overcome. These are points in which a considerable knowledge of biology might be of assistance to pathologists.

While it is impossible to deal here in detail with every kind of tumour, something may be said of embryomas and their malignant forms. Obscure and difficult as the subject is, there seems reason to believe that when it is understood many of the basal problems of biology will be solved with it. That they are due to some ovum, or embryonic ovarian tissue, developing parthenogenetically, seems more than likely. Shattock's remarkable paper on these tumours supports the view that embryonic ova may be fertilized by errant spermatozoa; but there are many reasons for coming to the conclusion that an embryonic "rest" may develop without such assistance. Such views do not account for infantile feminine or testicular embryomas. It is more likely that any epithelium in regions where reproductive processes commence may, under some abnormal stimulation, develop incompletely determined epithelial products or rudimentary organs. The prodigious fertility of embryomas in such products suggests that the imperfect parent tissue is doing its best to be normal, if the phrase is permissible; but that such a result is impossible, owing to the necessary lack of normal excitation and inhibition, *i.e.* of the usual environment. That a simple product of epithelium, such as hair, may be perfect is not surprising. The epithelium from which it grows is practically the only environmental stimulus it requires. That teeth, on the other hand, are rudimentary, misshaped, and monstrous, may be regarded as the result of their lacking a normal environment. That embryomas are frequently very deadly is what may be expected from the possibilities of the unspecialized tissues from which they originate. The study of interaction of the various tissues should include far more than the endocrine organs, since it is more than likely to solve the problems of heredity, as well as those of malignant growth. The divisions between physiology, pathology, and biology are responsible in a very large measure for the slowness with which they all advance.

It follows from all these considerations that it must not be supposed that reaction against one kind of overgrowth or the other is due entirely to the tissues principally concerned. Such a view would be a partial denial of the entire independence of the whole organic federation. There is reason to suppose that the blood-stream is hostile to intrusive epithelium. Small cancerous emboli excite thrombosis, and are sometimes buried, and perhaps destroyed, in a blood clot in which lymphocytes are probably very prominent. The erosive agent of the chorionic villus is in its multi-nuclear cap, or giant cell, sometimes without warrant called a plasmodium. Properly speaking a plasmodium consists of fused cells, and there is reason to suppose that a giant cell is one which accumulates nuclear material without normal fission. If we regard the nucleus not as a " director," which is a common psychological fallacy, but as a workshop containing the non-living tools, weapons, or catalysts, by which the cytoplasm works, it is easy to understand that when there is active use and much waste of such tools, mitosis does not occur. In normal gestation, when uterine reaction is complete and erosion ceases, there is probably no longer any multi-nucleated cell, for where such are found pathological conditions exist. If we knew when such a cell is again formed in fragments of the decidua we should be able to point to the very moment when chorion-epithelioma starts. It begins when the uterus has involuted, and is no longer in its highly developed and vascular form. I say highly vascular because, as remarked before, it seems that the blood plasm itself exerts a direct inhibitory influence on malignant cells. When considering this aspect of the problem I came, independently of any suggestion, to the conclusion that in some carcinomatous conditions I should expect to find multi-nuclear epithelial cells, closely resembling the cap of the trophoblast. This inference was confirmed by Mr. Sampson Handley, who told me, not at all to my surprise, that whereas no such cells are formed at the distal part of a carcinoma while still advancing in the lymphatics, they are to be found as soon as the growth comes in contact with the blood. This implies that there is a new reaction in the growth, and such a reaction seems obviously due to the inhibiting action of the blood-stream and the catalysts it carries.

If these conclusions are of any weight, and it is allowed that malignancy is a failure of developmental machinery, we are impelled to ask if there is any one gland in the human body, for instance, which, on the principles of interactions between epithelium and connective tissue, may be more to blame than another. The thyroid is suspect since it is frequently in a morbid condition in malignant states; but no proof has been adduced of its responsibility. There are, moreover, much greater reasons for suspecting another gland, directly responsible for definite under- or overgrowth, such as the pituitary body, since its direct connection with bone development is now admitted on all hands. But if the pituitary can determine infantilism, gigantism, and acromegaly by overgrowth or failure of bone-growth, and growth in all tissues generally, it is hardly extravagant to suggest that it may be directly responsible for bone sarcomas. If this is true we might then call sarcoma of the bones "local explosive osteomegalies." If there is such a thing as anarchy among the osteoblasts and osteoclasts, in which each under abnormal stimulation functioned regardless of normal inhibitions, we should expect such phenomena as we see in femoral sarcoma. It is at least a possible explanation to suggest that the deadly character of such a sarcoma is due to a breakdown in a bone exposed, perhaps, to greater single stresses than any in the body. But if the pituitary can influence one form of connective tissue, however highly specialized, it may equally influence other forms. From one point of view, the beginnings of all late sarcomas, not only those of bone, might be regarded as cases of overdone repair, while those of early life might be looked on as the result of over-stimulated activities in cases which under other conditions might have been the subjects of gigantism. Any glandular secretion may very conceivably have abnormal local effects. But, if early or late hyper-pituitarism has relation to sarcomas, hypo-pituitarism may equally well result at any time of life in under-inhibition of epithelium, a state, according to the theory here supported, which seems a necessary preliminary to cancer.

While I offer these views with the greatest diffidence, I may remark that though their acceptance might lead to further discoveries, and even to cure, their rejection would by no means invalidate my conclusions as to the relations of the two great tissues concerned. A breakdown of one or the other may conceivably be determined by any disorder of one or more of the endocrine glands. Such suggestions may, at any rate, lead to definite observations of the pituitary being made in fatal cases of malignancy, a thing, so far as I know, which has not been done. If any changes in it could be detected, they might offer a rational basis for therapeutics, though it is highly probable that a morbid secretion without visible change might be responsible for invasive tissues.

To put aside such speculations for a moment, and return to the general aspect of the problem, it must be owned that the confusion in theory is more than equalled by that in experimental therapeutics. Outside of surgery, which with radiology seems to present the patient with his only chance of prolonged life, most attempts at cure appear quackery, or empiricism run mad. Preparations of all kinds of tissues have been injected, and the results discredit optimism itself. Yet if it is agreed that the fundamental principles of life are inter-organic stimulation and inhibition, and that want of order is the result of failure in metabolic regulators, such views lead at once to considered experiment The arguments used to establish this theory may be deemed insufficient as proof yet, if they lead to trial, verification may follow. Such trials should be directed to assisting the reaction of all connective tissue in cases of carcinoma, and that of epithelium in those of sarcoma. How this can best be done is for the physiologist, radiologist, and pathologist to determine; but even if the suggestions as to the pituitary prove to be without foundation, it may be suggested that after the excision of a carcinoma efforts should be made to irritate or stimulate the connective tissue in the neighbourhood of the removed focus. We know now that this may be done by radiation, while the injection of doses of epithelial juices might assist the process. Since many forms of cell proliferation are inhibited by their own products, it is not unlikely that aberrant epithelium may be rendered inactive by injections of healthy epithelial products, or by prepared and filtered cancer juice. With sarcoma similar trials might be made, and in the meantime, while such experiments are in progress, on operated or inoperable cases, it should be the task of the physiologist or bio-chemist to separate from epithelium and connective tissue the chemical compound, or complex of compounds, by which they exercise their direct influence. Difficult as such a task may prove, the labour might well be worth undertaking, when we consider its possible results. In any case, much might be learned by the further study of normal epithelium and connective tissue in nutrient media, while they are subjected to X-rays or radium, or both, or to the influences of various endocrine secretions or toxins. Malignant tissues *in vitro* should be observed under similar conditions. We might then learn how and why certain epithelial cells become multi-nuclear, and whether such can be inhibited by the products of connective tissue, or of lymphocytes or lymphoid tissue. Even if little were learnt, a result I refuse to contemplate, the result would be that one field of research had been worked out on scientific lines. Such research, however, would almost certainly suggest that these diseases are indeed diseases of development, and must be combated by rendering the organism stable rather than by seeking any single cure, although it is by no means impossible that some simple and direct cure may be found. If it were discovered that some drug or drugs could stimulate or inhibit epithelial and connective-tissue growth the results might be of the greatest service.

If the results provisionally arrived at are summarized, it may be said that:

1. The general biological conception of the organism as a federation of organs and tissues, living in symbiosis, and yet fundamentally hostile, or "selfish," is helpful in the study of disease.

2. If atrophy or hypertrophy of the endocrines accounts for certain disorders, the failure of normal relations between less specialized tissues may account for others.

3. Order does not exist without control, and the essence of malignancy is lack of control.

4. There is reason to suppose that epithelium and connective tissue influence and control each other, and that their failure to do so is the real cause of malignancy.

5. Irritation, including the effects of infection, acts by destroying such balanced action.

6. The phenomena observed in the chorionic trophoblast, in chorion-epithelioma, in X-ray dermatitis and cancer as well as the experimental growth of the two tissues liable to malignancy, support the view of this relation-

ship between epithelium and connective tissue, and suggest that a morbid condition of the pituitary may be a fundamental cause of the disease.

7. Malignancy is thus brought into relation with the phenomena of growth, and can be classed with developmental diseases, such as those due to endocrine atrophy or hypertrophy.

8. Research should be directed to the discovery of the tissue products or secretions by which epithelium and connective tissue preserve their individuality and prevent reversion in each other.

REFERENCES.

ADAMI, J. G.—" Medical Sidelights on Evolution," 1919.

BAINBRIDGE, W. S.—" Cancer Problem," New York, 1914.

BAYLISS, W. M.—" Principles of General Physiology," 1915.

BLAND-SUTTON, Sir JOHN.—" Tumours," 1918, Lecture III., *Chem. Journ.*, vol. xxx. pp. 186–208.

BUTLIN, Sir H.—" Unicellula Cancri," 1912.

DARIER and WOLBARTH.—Quoted by Hartzell, M. B., " Diseases of the Skin," 1917.

ERLICH, P., and APOLANT, H.—" Mouse-Tumours," *Berlin Klin. Woche*, July 1905.

GREEN, C. E.—" Cancer Problem," 1919.

HANDLEY, SAMPSON.—"Melanotic Sarcoma," Hunterian Lecture, 1907, *Lancet*, London, 1905, vol. i. p. 909; *ibid.*, 1907, vol. i. p. 930; " Paget's Disease," *Brit. Journ. of Surgery*, vol. vii. No. 26, 1919.

HERNAMAN-JOHNSON, F.—" Comparative Value of X-rays and Radium in Malignant Disease," Aberdeen, *B.M.A.*

KETTLE, E. H.—" Pathology of Tumours," 1916; Path. Sect., *R.S.M.*, April 15, 1919.

KNOX, ROBERT.—" Radiology," 1918.

MURPHY, J. B., and STURM, E.—*Journ. Exper. Med.*, Jan. 1919, iii.

PAGET, Sir JAMES.—" Lectures on Surgical Pathology," 1853.

Russ, Sydney, and Chambers, H.—"Exper. Studies with Small Doses of X-rays," *Lancet*, London, April 26, 1909.

Shattock, S. G.—"Ovarian Teratomata," *Lancet*, London, 1918, vol. i. p. 479.

Spencer, Herbert B.—"Lettsomian Lecture," *Brit. Med. Journ.*, London, Feb. 21, 1920.

CHAPTER III

REPAIR IN EVOLUTION[1]

THAT dissatisfaction with much orthodox biological opinion is growing can hardly be denied. Not a little of this feeling is due to the fact that what is often given as explanation cannot be resolved into factors capable of appreciation, and, possibly, of measurement by the intellect. The theory has to be accepted as more or less a matter of faith, and the very definite relations of biology to the allied sciences are almost entirely ignored. If the views advanced in the previous chapters carry any weight, this alone is sufficient to account for discontent. Where there is a general tendency to rely on authority, speculation is discouraged, for orthodoxy everywhere rests on the native conservatism of man, and even the revolutionary is at last capable of fatigue. As a result, tentative hypotheses offered by the great leaders tend to become objects of worship, and among their less enterprising followers there arises a more or less fervent conviction that, however unsatisfactory they appear now, they will presently become demonstration. Thus the theory of the germ-plasm, even in its later modified form, seems held too dogmatically by many: the "nature" of

[1] V. "The Function of Pathological States in Evolution," *Zool. Soc. Proc.* 1918. The paper has been added to and altered.

inherited living matter accounts for every organ as it appears; while all changes are due to obscure variations of an advantageous kind which give the survivors in the struggle a better chance. On analysis, such opinions do not seem truly scientific, for the "nature" of the germ-plasm can barely be distinguished from the directing entelechy of Driesch, and if the Weismannian cloud of ids and biophors is now somewhat condensed, the magic determinant still remains in a concealed vitalism which is exactly analogous, as regards the organism, to pantheism as regards the universe. Nor, if we are told with certainty that altered characteristics are not transmitted, is the theory of small advantageous variations much more satisfactory, if we know neither how they come, nor how they are inherited. To say so much must not be regarded as treating with disrespect its great author, without whom we might still be wandering in the barren field of teleology.

To regard these theories as hasty and, perhaps, unsound explanations is not to accept without scrutiny the theory of the transmission of acquired, or modified, characteristics. Though this is a view that can be defended on the physico-chemical grounds of catalysts which are measurable determinants of a really scientific order, experiments to prove the fact must take a very long time, and we are compelled to rely on other methods of proof. That the experiments of Tower and Kammerer, for instance, suggest the transmission of modifications cannot be denied. Such as oppose the general view that the environment has thus an inheritable moulding influence on the organism, seem to reply that those are only rare and doubtful cases, whereas the theory of inherited advantageous variations, whether continuous or discontinuous, can be made responsible for the whole of the phenomena. As the conclusion is gradually being strengthened that large variations of a Mendelian character deal with other characteristics than those which are racial, all who rely on inherited spontaneous variations are forced back on the Darwinian view that small variations can gradually, if of an advantageous kind, convert one species into two or more, and that all living characteristics, or organs themselves, are due to such a cumulative effect. It is, of course, inferred and definitely stated by Darwin, that any variation in the least degree injurious would inevitably be destroyed. It is this statement I propose to examine, and for the purpose of such an inquiry it must be clearly understood what is meant by the word " disadvantageous " or injurious.

At first sight nothing seems clearer. Why should we doubt that any functional or organic failure is a handicap in the biological race? By functional trouble, of which the cause is not obvious, we mean some hindrance, which may be recovered from, to normal or physiological action. It is due to factors which, for the most part, are unknown. We do not doubt that there is a failure somewhere, which, as regards certain cells, might be called organic, but often we cannot do more than guess where the actual failure occurs. In that advanced disorder of function which has visible lesions, and destruction or irremediable alteration of the individual parts of the machine, there is undoubted organic disease. Can anything seem more certain than the conclusion that any organism which fails in the established functions of its species is as a fact severely handicapped, that the variation is disadvantageous, and cannot possibly be transmitted either directly or by survival? There are, however, some reasons for believing that this inference is inaccurate, and that the function of disease in evolution is of much greater importance than that of mere elimination. But pathology has very naturally been neglected as a study by biologists. On the views generally held, it has seemed sufficient to recognize that disease destroyed organisms which obviously left offspring, if it left them at all, that were handicapped even more heavily than their parents. It has been understood that their elimination was only a matter of time, and that neither their virtues nor their failures could influence the race.

If there is one thing more than another which has struck me when attempting to study these questions, it is that too many men of science appear to believe that any serious investigation of other branches than their own is for them a waste of time. In no case is this more common than in that of the biologist, who yet, by the very name and nature of his task, should include in his apparatus a considerable knowledge of everything which deals with the organic, and even inorganic, world. Science, however, is kept in more or less water-tight compartments, and it seems left to the mathematician to hold the opinion that his own branch of learning has, somehow or another, deep relations with all things, including life itself. Even by him it does not seem to have been pointed out that in things living and non-living certain principles of construction rule alike. However much they were wedded to mechanico-physical explanations, biologists have assuredly often ignored the fact that any organism is construction, and knowing little of the laws of construction have ignored basal facts familiar to every architect or even every artisan. It was reserved for Wolff, in formulating his law of bone-growth and reaction to stress, to propound a principle more far-reaching than he recognized, when he showed that living bone, reacting to normal or abnormal stimulation, can be proved to develop in accordance with the principles of engineering and architecture, although he apparently laid far too little stress on the action of muscle in bone transformation. This law may, I feel assured, be extended to every living tissue, and in such an extension will be found the key to many phenomena still awaiting explanation.

To one who holds this view, the work lately done by Starling on the *Law of the Heart,* which shows that the force with which the heart contracts is directly proportional to the length of the muscular fibres at the end of the preceding diastole, is by no means surprising. It is indeed on a par with the conclusions of Wolff as regards bone, and might, I believe, have been deduced from it or from the form I suggest, provided it is understood that each varying tissue has its own acquired typical reaction.

If, then, it can be shown that disease has had a profound effect upon the evolution of all organisms, and that analogous results are found in every kind of human constructive effort in such numbers as to suggest as a law that all great variational developments result, not from the happy-go-lucky aggregation of small advantageous variation, or from discontinuous variation, whether of a Mendelian character or not, but rather from partial failure and repair, we seem to be in sight of a general principle of profound importance. If this principle proves sound, it is obvious that immense labour has been spent by biologists endeavouring to explain life without seeking help from other workers. Though they may show some general knowledge of the cell, and even special knowledge of the reproductive cells, I find few who appear to have studied general embryology, to speak only of one branch of physiology. On the other hand, many physiologists and pathologists have done good work in some branches of evolutionary theory. Bland-Sutton, in his fruitful little book *Evolution and Disease,* pointed out that, "Pathology is only a department of Biology, and it is important to bear this in mind in studying disease." It is true that he went little further than to show that what is pathological in one organism may be physiological in another, and that many diseases are reversions, that is, failure in normal growth. Yet this greatly needed to be shown, and it is not to be expected of a great pathologist and surgeon, and, perhaps, the less the greater he is in his own branches of work, that he should attempt tasks from which many of the biologists themselves seemed to shrink. Claude Bernard made similar remarks as to pathology. It is to be regretted that a stumbling-block was placed in the path of progress by Darwin's hopeless dictum as to the explanation of variation, just as another was by Huxley when he declared consciousness an insoluble problem. In every science great discoverers have too often delayed progress as much by authoritative unsound opinion as they have advanced it. Every Bible is first a book of revolution, and then a refuge for reaction. Yet no man can possibly know all he should know for the purposes of his own work. This fact affords the only justification for those, who cannot pretend to profound knowledge in any special line, attempting to solve problems which by their nature are beyond the specialist. They may have been able to grasp in a measure the general conclusions of each science, and by a happy, perhaps accidental, combination, show at least part of the forest to those more particularly occupied with the trees themselves, or the flora of the undergrowth.

It is remarkable that hitherto no one seems to have made the observation that reaction to an actual, or threatened, breakdown is one of the basal laws of all construction and organization. Yet none can read engineering without observing that all development has followed such lines. As new stresses are introduced, failure is threatened, and steps are taken to obviate disaster. What is a patch on one engine becomes organic in the next. Since waste of energy can be looked on as pathological, we observe the reaction in the engineer against such failures, as the atmospheric engine is succeeded by improved forms ending in the quadruple expansion engine. Many other instances could be adduced in general or special engineering evolution; but the best illustration of the facts which need elucidation can perhaps be found in Gothic architecture. If such a demonstration of this general principle can be made it will go far to obviate the objection, very likely to be urged, that what occurs in human construction has no relevance to the living organism, especially if it can be suggested forcibly that human intelligence is in itself a reaction, and that the law obtains in developments of all kinds. It is, indeed, not going too far to declare that there is no real qualitative difference between the cytoplasm of a test-bearing protozoon as it elaborates its peculiar envelope and the general cerebral protoplasm of a human community constructing some great edifice. That trial and error are at the base of evolution is indeed implied in the current teaching as to variation, and its extension to intellectual processes will surprise no worker who has had to deal experimentally with the unknown. We may expect, but never know, where to look for failure till we see it. When it is seen we can do our best, as reacting agents, to remedy it. Having said so much, and leaving aside the wider implications of such views, we may turn to such a problem of construction as the evolution of a cathedral, in the hope that it may throw a light on other than architectural puzzles: merely observing, on the way, that no general principle yet discovered is confined in its application to one branch of knowledge. Having once found it, our task is to employ it as a weapon of further analysis.

It is more or less a commonplace that function creates structure, however Lamarckian that may sound, and in the case of architecture of a religious order the function which constructs is public worship. In fine climates the necessary structure is often a roofless temple. In tropical climates a flat roof may be needed as a protection against the sun. In temperate climates a walled enclosure is insufficient, and a flat-roofed structure cannot keep out rain effectually or bear heavy snow. Thus arose the pointed or sloping roof. But it has been said that " Gothic architecture is not a style. It is a fight." The arch is a mighty warrior. It gives and receives thrusts. The sloping roof partakes of the same nature. Need created it, and the nature of materials and the positional energy we call gravity caused thrusts which endangered the simple walls of the building, walls at first meant to support nothing but flat roofs probably covered with brush or the like material. To build stronger walls might have occurred to the primitive architect, but as the danger was immediate, he probably at once shored those in existence, and then built others at a right angle to act as buttresses. In the meantime the worshippers increased in numbers, and it is indulging in no flight of fancy to suppose the later builder saw that if the new external walls were roofed over, and doorways cut into the main building, there would be an immediate increase of space by the creation of chapels. Such a series of embryonic additional walled spaces, with further doorways in them leading to each other, obviously gave him the aisles. The flying buttresses, which are such a feature in great Gothic architecture had, I can only suppose, a like origin. They were originally buttress walls carried up to the roof. At some period a genius, already acquainted with arcuated structure, saw that if the inside of these walls was cut away, they would still take a heavy thrust and lighten the rest of the building. If, however, on being converted into such slender stone shores they showed signs of yielding, what could be easier than to pile some of the removed material upon the base of the flying arch, and thus create the beginning of the pinnacle? Though an architect might develop such a rough statement, he would be the first to admit that it represents in few words much of the evolution of a church: that is, he would own the structure sprang from need, and that each new need caused a constructional failure which, when strengthened and corrected, was the cause of further structure. He would further tell us that all good ornament is organic; that it springs naturally from the work already done, being in its origin just the little more needed to give a margin of safety, though on it later are exercised the æsthetic faculties of man, which are again a response to the need of full satisfaction for the instinct of workmanship. Human ornament is in fact strongly homologous, if we may use that word here, with the beauty of very energetic birds, who carry out by virtue of their free energy the extension of structures and colours already existing in their less brilliant forms. That, however, is by the way. The main fact we are concerned with is that the building as a whole is evolved through trial and error, through failure and repair, through a threatened structure to a more complete and adequate one for increased function. In a word, the great origin of structure was failure after failure duly compensated for. Is there any reason for believing that variation in the structure of living organisms follows exactly the same principle? Are we entitled to say that the mammal, for instance, with all its complexity, is the result of infinite ages of functional failure or disease which was met by processes of repair and reaction? In a word, can we speak of the evolutionary value of disease, of impaired function, of disadvantageous variations? It seems possible to do so, if what is true of one structure is roughly true of another.

It may seem absurd to talk of the value of disadvantageous variation; but it is no more absurd than to imply that all variation is advantageous because it is perpetuated. What is useful at one period may be harmful at another, and embryologists thoroughly understand that developments useful in fœtal or larval life may open up many dangers for the adult. The real point to be considered is whether organisms as a species do not vary and run great, even largely destructive, risks by an increased pressure of function which, in the few that finally react, or whose descendants react, to such stress, results at last in structure that is advantageous *as altered.* The given variation in itself may be a failure of what was normal function in the species, and we should therefore as pathologists or physiologists speak of it as a disease; but if the few that recover become a new species, a mended race, it is no longer disease. After many generations it may be truly advantageous to individuals. Have such processes occurred in the evolution of organisms, as they undoubtedly have in the arts and social progress, where we often observe political failure of organization result in *ad hoc* reaction which leads to a changed social form? I have no doubt that they do, and many organs in mammals, to speak only of them, show it. It is, in fact, a universal principle. As beavers patch up a dam when it yields or threatens to give way, so tissues, organs, and societies react to threatened disaster. In no tissue is this clearer than in bone. It is true that Wolff's law only deals directly with mechanical stresses, since it runs: "Every change in the form and position of the bones or their function is accompanied by certain definite changes in their internal architecture, and by equally definite secondary alterations of their external conformation in accordance with mathematical law"; but I hope to show reasons for concluding that such a law may be stated in more general terms, and applied to every tissue and organ, provided we add, as suggested before, that the more complex the tissue or the organ the greater the liability of failure, and that each tissue reacts in a typical way.

It is unnecessary to go into details of osteogenesis and morphology. It has been recognized by engineers that the head of the femur is formed exactly in accordance with mechanical law. Had any of them been required to design a structure fit for undergoing the stresses borne by the femur in its development and after-life, he would have sketched a figure extremely like it, not only in its general shape, but in the trabeculæ which support the bone in every direction where extra stresses are applied by normal function. The important point to note is the fact that femoral development follows stress in individual development, from which we must draw the conclusion that it followed stress during evolution, not that its value for complex function was gradually increased by chance or "spontaneous" variation, unless we attribute to "spontaneous" a meaning which Darwin never gave it, seeing that he denied knowing how variation arose. All the variations were definite responses, and it is easy to infer that before response became rapid and easy every kind of disaster and disablement must have occurred to those subjected to reaction-provoking stresses. The very process of adaptation (and on these lines "adaptation" is no longer a mystic word) implies long periods of disordered function and poor structural response even in those who survived after repair. But now bone is so plastic and fluent that when it is grafted the osteoblasts and osteoclasts use and shape it according to the form of the main bone of which it becomes a part. For, according to Keith, Wolff's law may be more simply expressed if we say: "Osteoblasts at all times build and unbuild according to the stresses to which they are subjected."

When we speak of repair it may be noted that the treatises on this subject are strictly limited in their purview. They mostly follow Hunter, a vitally important figure in the history of pathology, and indeed of all medical science, who, however, lacked the apparatus of knowledge now at every one's disposal. We learn a great deal about the repair of wounds and fractures: of the functions of the fibroblasts or of the wandering cells of the blood-stream, and are told, lately, much of regeneration; but of the evolutionary value of organized exudations we hear little or nothing. Nor has it been suggested that it is to this and analogous processes that much new structure is due. That this is so is strikingly apparent, as I shall attempt to show, in many organs of a highly specialized type. In no structure, perhaps, is the process so clearly seen as in the mammalian heart, which is a perfect museum of evolutionary failures and dislocations, compensated for by an extraordinary complication of patched-up tissues and altered muscle in which, perhaps, one tissue takes on the functions of another, and some evolutionary remnants long survive without function. I was, indeed, first led to take this general view of the variational value of pathological conditions by observing that the heart, when laid open from any aspect, powerfully suggested an organized or cured aneurism. By this I do not mean that it is now in any way aneurismal, or that the heart is descended from such a large and definite breakdown. The view put forward is that the complex machinery of the *chordæ tendineæ*, the *columnæ carneæ*, the *papillares musculi*, the moderator band and the valves generally, gives it the appearance of a repaired organ, and inevitably suggests that, during its evolution, fibrosis and the reactions of stressed tissues moulded and re-moulded it on the general lines of mechanical construction, breakdown, and repair. Many must have made the same observations, even if they have not come to similar conclusions. The anatomist and pathologist perhaps know their subjects too well, and are necessarily greatly dominated by current theory. The general adaptation of the heart to the work it performs may well delight the anatomist as he studies its machinery. His main business is not evolution. The pathologist, on the other hand, observing its many failures, is scarcely likely to discern that by failure itself may come eventual perfection, and while the physiologist considers its functions rather than its apparatus, he studies it as it is, not as it was. In each case the observer may not see the forest for the trees. Yet when we look at the partially repaired aneurism with its fibrous growths, and turn to the opened heart, the essential likeness of the *chordæ tendineæ*, for all their definite functions, to the rude fibres of an aneurism, is obvious. Is such a likeness an accident of evolution and pathology, or are we to consider the heart as much an organized dilatation sac of the whole fused circulatory canal as the cured aneurism is of a part of it? It is in embryology that we seek for confirmation of what is suggested by anatomy. But even anatomy alone offers powerful proof of the view that the heart, as we know it, is the latest result of repeated failures of the circulatory canal under strain, and of the repairs effected by the stressed tissues in their response to changed and abnormal stimuli, just as bone alters under its particular stresses. During embryological life there is found in the heart a small patch of non-functioning muscle in the anterior segment of the mitral valve. Its presence is intelligible if we consider it a relic of a disrupted and repaired organ. The muscles of the heart are obviously homologous with those of the arteries. Yet they have become striated although they are, of course, still involuntary. Non-striated muscle is the earliest in evolution. It seems that the increased functioning of the cardiac muscle has converted it into its striated form, so that it resembles skeletal muscles, which are much more active than non-striated muscle. The whole histology of cardiac muscle probably represents the result of great strains. Structures such as the disks or bands of Ebarth are found nowhere else, and may be the result of peculiar stress. There are even portions of muscle which no longer perform muscular functions. Their fibres do not contract, but serve instead to conduct stimuli as if they were nervous tissue. All tissue is conductive, but the Bundle of His, with its Purkinje fibres, which carries the impulse from the auricle to the ventricle, transmits messages at ten or twelve times the normal muscular rate. When it fails there is heart-block. In the embryo the valves arise from the cardiac walls, and are composed of muscular tissue, which by the action of fibroblasts gradually become non-muscular. This must have been originally a pathological process. It is a reversion, a degeneration made use of. We observe analogous, or shall I say homologous ? results in the hypertrophied heart. The normal male heart weighs about eleven ounces. In some cases of aortic stenosis it may weigh over thirty ounces. In such hypertrophied muscle are often found fibrous tissues which probably represent the connective tissue of muscular fibres which have atrophied from overstrain. The attachments of the mitral valve are less muscular and more fibrous than those of the tricuspid. The greater elasticity of the tricuspid *papillares musculi* and the annular muscles of the base of the ventricle thus allows an overfull right ventricle, which is so much less powerful than the left, to be relieved by the temporary functional incompetence of the tricuspid valve. In the reptile with a functioning *foramen* the valves are purely mechanical, as pressure is relieved by the patent orifice. The *fossa ovalis* in the mammal is a remnant of the early communication between the auricles. In a large number of normal hearts there is a small valvular passage yet remaining in the left margin of the fossa. None of these phenomena seem capable of explanation as the result of spontaneous variations arising from some theoretic instability of the organism. To argue that they are is to give biologic mystics a chance. It appears obvious from all these facts taken together that cardiac evolution has been a series of caused variations due to increased and varying stresses, which acted not only as a moulding force on the shape and musculature of the heart, but on all its appendages. In the muscle of the ventricular walls with its extraordinary complexity of layers and interlaced fibres lies powerful evidence of such reactions. In both ventricles there are seven muscular layers, while in the arteries there seems but one. In the left ventricle these layers are obviously thicker and stronger than in the less stressed right cavity. But how did the ventricular cavities acquire more layers than the arteries? No new muscle fibres arise after birth, and yet there is obvious reason for believing that stress can be responded to by increase of muscle fibre during evolution. In the gravid uterus the smooth fibres of the wall increase to eleven times their normal length, and are from two to five times as broad. So far as we know there cannot be new fibres in it. But in evolution new fibres are undoubtedly found. In the arteries, the fibres of non-striated muscle in the *tunica media* are for the most part circular, but they appear to have more or less longitudinal branches which interlock with like branches of the neighbouring fibres. One of the most prominent features of an individual aneurism is the thinning out, and sometimes the disappearance, of the *tunica media*. The muscle fibres in such cases are completely broken down, and if the aneurism is repaired in individuals the work is done mostly by an increase of the connective-tissue elements. The process is said by some to be a reparatory endarteritis, in which the tissues of the *adventitia* proliferate actively. But the evolutionary process has obviously taken the path of increase and reactive proliferation of the muscular elements of the *media*.

Without attempting a task of which I am incapable and endeavouring to elucidate the problem of the origin of circulatory systems in a primary vascular sponge-work, through which the fluids of the primitive organism were propelled by contractile tissue, it may be noted that the *columnæ* and *chordæ* rise from such a sponge-work which, at an early embryonic period, fills the primitive ventricle.

To interpret such an origin and their present functions it seems they must be looked on as reaction products found useful when the chambers of the heart arose as dilatations of the primitive tube. Such dilatations were probably, I would even say certainly, failures of the walls. The incomplete pathological disaster of a repaired aneurism helps us to understand such evolutionary failure and repair as enabled the evolving heart to endure greater stresses, and be once more repaired. It may be added that the sponge-work of the evolving primary ventricle is strictly analogous to the vascular spongy tissues seen in the male organ. Every pathologist will admit that such a structure may be logically compared with a vascular aneurism. The path laid down by pathology is trodden by physiology. It follows that during evolution there must have been an immense destruction of organisms whose circulating canals did not react, and numbers which retained their unaltered "specific" characters. The same process goes on to-day. Though many die of cardiac disease, it may be that much youthful functional trouble, and even more serious adult disorders, are even now re-moulding the heart. No organ is perfect; if it does not degenerate it progresses. Though such processes are "disease," it by no means follows that they will be destructive, any more than that the functional incapacity of the tricuspid valves in athletes, which probably precedes what is known as "second wind," is anything now but a cardiac safety-valve.

As we learn more of the heart and its latent capacities we may, perhaps, say with the late Dr. H. G. Sutton, "we trust nature too little, to say the least of it." But there are, of course, great difficulties to overcome before we can hope to understand how the cardiac musculature has altered, and may still be changing by the addition of new fibres.

As yet, little is known of myogenesis. Like a neuron, a muscle cell seems to last a lifetime, and though both may degenererate or die, neither proliferates after the early period of development. But whatever their histogenesis, new fibres do appear in evolution. Harvey did not refuse to believe in the validity of his own conclusions, because he lived before Leeuwenhoek. With considerable hesitation I venture to suggest that morphogenetic stress is at its height during fœtal development. The child *in utero* has not, perhaps, the calm and happy life commonly attributed to it. On the contrary, it probably leads a strenuous existence, and if it inherits a new weakness this is shown just where and when new stresses find plastic embryonic tissues to respond to them. If such a speculation is sound it accounts for many phenomena. But in any case, whatever the machinery of inheritance and evolutionary repair, it is certain that new fibres arise where they are needed.[1] The origin of the *cremaster* muscle as a lately evolved support for the testis certainly strengthens this view. Hunter could not account for its appearance during embryonic life, when the testis occupies its original position, and the *cremaster* serves no purpose. In the *testiconda* such a muscle is not found. It must obviously have arisen as the result of stress during the evolutionary descent of the testis, and cannot be accounted for except by such stresses and fœtal hormonic influences.

If such views in any way represent the biological history of the heart, it is obvious that many of the opinions of variation usually held are without foundation. Every variation is definitely caused; it is in no sense accidental or spontaneous; it may not even be at once advantageous to the individual; on the contrary, it may be a severe

[1] See Appendix B. The *Peroneus Tertius.*

handicap which puts greater general stress on all who experience it, though such stresses fall short of those which cause death. Variations of this order may only be advantageous to the whole species as a continuing race. They may destroy, and doubtless have destroyed, individuals without number at an earlier age than the usual life-period of the unvaried type. We may possibly imagine a part of humanity, now responding to stresses which make the heart do more work and fail earlier, displaying such energy during their shorter life as to displace those with a normal cardiac mechanism which survives to the average age of man. It is to be inferred from these considerations that the structure of an organism is not a congeries of minute fortuitous advantageous variations, nor the gradual massing of details in an orthogenetic line, nor the result of large discontinuous variations due to chromosomatic inheritance, but a complex of definite reactions to definite stresses. The true theory of living structure is that its growth is neither casual nor foreseen, but that it is what we may call, in political language, the " opportunism " of the organism as a whole. Every advance is a forced, even a desperate, experiment. Life, like a hypothesis or a dam, is built up by stopping leaks.

The evolution of the stomach seems to have followed the lines suggested for cardiac development. From the physiological point of view, an intestinal tube which becomes dilated cannot be considered anything but pathological. It has failed under the stresses on it, but the organism which reacted turned a weak dilatation sac into a strong permanent food pouch. The results to the reacting organisms were many. The ingested food became temporarily static, was more thoroughly dealt with, and the organism was not continually feeding. Its whole available energy was not devoted to nutrition; it had time at its disposal, and could develop other functions leading to further structures. That the mammalian stomach is such an organized failure is suggested forcibly by the musculature. In the small intestine this is composed of two layers of fibres, circular and longitudinal. In the stomach it is made of three sets, an inmost layer of oblique fibres being added. This oblique layer is obviously a later growth and, as would be expected on the lines laid down as to disaster and repair, its strongest fibres are found supporting the greater curvature or dilatation of the stomach. When speaking of these muscle fibres, it is, of course, understood that they not only resisted the passive strains of ingested food, but also exercised their active basal function of contractility as well. This later oblique layer is naturally less well developed than the longitudinal and circular fibres. Other oblique fibres are formed about the pylorus, where they form the sphincter. I suggest that these oblique muscle fibres arose at points of strain, under intense stimulation. The dilated pouch has reacted in accordance with mechanical law, just as the heart did with its more complex arrangement of oblique fibres woven into a structure capable of giving in the left ventricle a thrust of over fifty pounds. The reacting organism is no fool of a mechanic either in its bones or its muscles, and these phenomena are additional reasons for extending Wolff's law to all tissues if it is understood that, while bone responds to gravitational and compressional stresses, and to the tensile stress of muscle, the fibres of muscle produce the very stresses to which they respond by increase of bulk and strength. If protoplasm did not so react there would be no problems to solve.

Such views on the mammalian gastric apparatus are so obviously supported by the embryology of the organ that there is no need to go into details beyond noticing that in the fourth week there comes the first dorsal bulging in the foregut. But if evolution is still proceeding, is it absurd to suggest that the common symptom of a general disturbance of health known as dilated stomach may be a pathological process actually in the process of becoming physiological ? According to some physicians, few modern stomachs do not suffer at times from an amount of dilatation which is pathological; *i.e.* the gastric musculature fails to react correctly. The stomach may yet be such a functioning dilatation pouch as to enable the human race to do with no more than one meal a day, or even less. Our descendants will have all the more time for work. This by no means implies that the empty stomach should be any larger than it is now in healthy subjects. Before the invention of X-rays the gastric apparatus was always pictured in text-books as usually seen on the post-mortem table. The dead stomach was shown as the portrait of the live one: the weakened pouch of the sick man as that of the live and healthy subject. But nowadays it is known that such extreme dilatation is natural only when a large meal has been taken. When the healthy stomach has emptied itself it has contracted so that it nearly resumes its ancient cylindrical character and then goes into a state of rest or relaxation. With further development it might hold still more, and yet react in the same way. The suggestion that functional failure or disease, which becomes organic and destructive in many, may, in reacting and surviving organisms, alter their outlook on life and all their activities, seems to me powerfully reinforced by these considerations. The disadvantageous variation does work, and finally improves the race. It is a big subject, not to be enlarged on here; but there still remains much work to be done as to the indirect influences of diseases, infections, and otherwise, upon physical and cerebral development. It may be suggested that the acuity of sensation and perception of those affected, but not disabled, by tuberculosis and the slow acquisition of immunity, may have modified human character to a marked degree.

It can even be shown that disadvantageous variations actually become permanent racial characters. We may consider hernias. During the processes of evolution, a mammalian hernia seems to have occurred almost universally, and to have established itself as normally physiological. The *tunica vaginalis* of the testis is actually part of the original peritoneal sac, as can be seen in the embryo. This was, of course, observed by John Hunter. During fœtal life it is separated from the parent sac. In whatever sense we now call such a change physiological, it seems impossible to regard it as originally anything but pathological. I certainly do not know how we can describe the scrotum as anything else than the coverings of an evolutionary hernial sac, which is not only of no advantage, but a positive danger to most male animals. This view has been supported by Bramann. In some, the pigs for instance, the testicles do not descend into an external pouch, but are supported and protected by the normal skin tissues, not by a thinned and delicate integument of later development like the scrotum, a tissue still scantily supplied with the non-striated muscular fibres which might have reinforced it, and are, perhaps, now developing slowly. When we consider the rarity of muscular fibres in human skin tissues in comparison with those of animals, their greater frequency in the scrotum and perinæum suggests that they are a reaction product, a forced revival of the primitive panniculus. They act in the *dartos*, or deeper layers of the scrotal dermis, at right angles to the rugæ, and are something of a support. The pink colour of this structure is due to the presence of these muscular fibres. They are not connected in any way with the *cremaster* muscle, and therefore are not affected by the cremasteric reflex. In many senses the descent of the testes cannot be called advantageous. It causes a weak spot, recognized as such by men and animals. The Japanese wrestlers are said to be trained from earliest youth to return the testes into the inguinal canals. If the translation of the testis from a safe place to an exposed one has had any good results, they have been indirect and only discoverable, though not yet discovered, over long periods during which the change must have been disastrous to many. To argue that they were advantageous to begin with is to destroy the authority of reason. It is true that at the present stage of human development an undescended testis rarely produces normal spermatozoa. But to argue that the testes descended because the rise of intra-abdominal pressure produced conditions incompatible with racial continuance seems to ignore physical causation in favour of a partially teleological explanation. It cannot be argued that their present situation was always the best, or that their early position was disadvantageous in face of the fact that in elephants, seals, and walruses the testes remain undescended, and that the boar's are at least inconspicuous and protected by normal skin tissues. As a rough partial illustration, it may be said that though an emigrant's descendants might do badly in his native village, it does not follow that they might not have functioned there successfully if their parent had not left it.

It may seem an undue extension of the view that pathology has played an immense part in evolution, if it is suggested that it was upon pathological conditions that the very existence of the Metazoa depended. There can be no doubt that they originated from some protozoon by a failure of normal physiological fission. We see here how theories of disease may be modified according to the point of view taken. From that, shall I say, of a protozoan Hippocrates or Hunter nothing can be more obvious than that a failure of mitosis would be a calamity, the birth of a monster, of Siamese twins, among the normally constituted unicellular organisms. It is still in the processes of reproduction that we find the strongest evidence of the part played by disease.

When considering such problems in this light, it seems somewhat difficult to account for the satisfaction of many with the theory of small cumulative advantageous variations. What ground is there for imagining such machinery could result in a complex series of adaptations such as the uterus, and what we may call its habits and customs in dealing with the embryo from the entrance of the ovum till birth? Even those who adapt to their own ideas some theory of large discontinuous variation will, in the end, be compelled to attribute the uterine growth and functions to a mystic power or virtue in the original germ. They may follow some philosophers, and "unpack" powers out of a conjurer's bag without telling us how they got there. Yet if we regard the uterus as the result of tissue reactions under abnormal stimuli, being guided in research by the processes seen every day in disease, the variations, whether small or large, continuous or discontinuous, assume an aspect neither fanciful nor mystical, and our need for biological faith is reduced to a decent scientific minimum. To say so much is not to deny that small variations may finish, or polish, a rough incomplete adaptation. From an eolith to the perfection of Chellean art may be such a process, but the first eolith was no small variation.

The fact that the embryo acts upon the maternal organism as a parasite against which the mother has to be protected, is commonly recognized, but I have not seen the obvious conclusion drawn that the whole history of the mammal must have been due originally to a pathological accident in some one or more of their ancestors. The mammalian animal still lays eggs, but they are not extruded. When such retention first took place, it must have been due to an accidental pathological delay of the travelling ovum, owing perhaps to catarrh of the tube. Even now the mother has to be rendered immune to the products of the offspring. Many of the phenomena of early gestation are those of immunization, in some cases a very slow process, as is shown in human beings by vomiting and *malaise.* It has, moreover, not been clearly or generally recognized, except by pathologists, that the very methods by which the ovum attaches itself to the uterine wall are, so far as the hostess is concerned, actually pathological and bordering on the malignant. Yet they have resulted in a series of protective reactions which save the parent and permit the growth of the parasite. The method by which the ovum becomes partially buried in the tissues is obviously of a destructive kind, and curiously analogous to the malignant processes seen in chorion-epithelioma. Bland-Sutton remarks, "This disease is instructive because the erosive action of the trophoblast is the physiological type of the invasiveness so characteristic of many varieties of cancer." It must, I think, be added, that it is the balance established by reaction which makes the trophoblastic action physiological.[1]

That the influence of the ovum on the undeveloped tube must have been of an exceedingly dangerous character is now seen in tubal pregnancies during which the chorionic villi frequently penetrate the wall of the tube, which does not react as powerfully as the uterus. Such a process in the uterus, which is itself a tubal dilatation, is now normal, because these villi, the earlier nutrition roots or organs of the parasite, are prevented from injuring the uterine wall irrevocably by the transformation of the reactive uterine decidua and the chorionic villi and the allantois of the fœtus into the combined temporary organ known as the placenta. It may be noted that the non-placental mammals are less exposed to the destructive and toxic effects of their offspring, as they are born at an earlier stage than in the case of the deciduate mammals. The marsupial fœtus is about half an inch in length when transferred to the milk-pouch. It is impossible to look at the placenta without recognizing that it is what we may call a compromise growth, one which serves the embryo without destroying the parent hostess. That all mammals are not yet fully armed against any morbid alteration of function in the penetrating chorionic villi is seen, as suggested above, in chorion-epithelioma, where the energy of the villi trophoblasts leads to a malignant overgrowth of the epithelial elements, which the maternal tissues fail to inhibit. The hydatid mole, which does not as a rule become malignant, is a case where such inhibition has been sufficient. These phenomena establish on a firm foundation the view that the uterus and its reactions during gestation are definite protective processes or variations springing originally from a purely

[1] See Chapter II., *Malignancy.*

pathological accident in some ancestors of the mammalians. However complex the embryology of the uterus and its appendages, the broad facts are compatible with this view, which is strengthened by the later parasitic history of the offspring after birth. The mammæ appear to be a compromise between the needs of the infant and the protection of the mother; it has been suggested that they originated in sore or tender spots on the epithelium most exposed to the assaults of the parasite. Whether this is true or not, the growth of the nipple is a complex variation depending on the mechanical action of sucking with a reaction proliferation of the epithelial elements of the sweat and sebaceous glands, and an increased blood-supply as special maternal protection against oral infection. It seems to me that few stronger instances can be found of the fact that the development of many organs, if not all of them, is the result of direct reactions or adaptations, which are in the nature of repair to tissues otherwise likely to suffer disastrously.

It is large macroscopic results of this order which enable us to reason about other finer reactions, and even help us to link to the general process those of a microscopic and ultra-microscopic character which we class under "immunity." Such phenomena are reactions under stress which, by the provocation of catalysts, influence life. If, indeed, much of human character is similar reaction, perfect or imperfect, to the infections to which the race has been and still is exposed, psychology itself must at last be classed as the result of physical reactions—a conclusion fully in accord with the work of Pavlov on conditioned reflexes.

If any further illustration of the conclusions so far suggested is necessary, it may be found in the fixation of the mesentery, and the changes undergone during its development. It has often been pointed out that the embryonic processes by which it is made secure are histologically those of plastic organized exudations, *i.e.* those which have been invaded by fibroblasts. These attachments do not come about at the same period of fœtal development, and it seems of significance when we note that the mesentery of the small gut has an oblique attachment, to the posterior abdominal wall from the duodenum to the right iliac fossa, only found in animals which have assumed the upright posture. This comes into existence as late as the fourth or fifth month of fœtal development. Before this band formed there must have been a great series of disasters, for even now the last part of the mesentery to become attached to the abdominal wall, that is, the angle between the ileum and ascending colon, sometimes remains free. A volvulus may easily form there by rotation of the ileo-colic loop. The whole history suggests a series of lymph effusions, caused by pathological states, some of which were sorted out by the lethal process of natural selection, the remainder surviving and leaving offspring with the liability to organize the effusion in the safe way. The pathology of those cases, in which what are known as Lane's Kinks can be found, is obviously of a similar character. The stasis of the affected bowel causes lymph effusion, and the formation of bands which are morphologically homologous with the early attachment of the mesentery. In Keith's paper, "Nature of Peritoneal Adhesions," I find noted the normal loose network of connective-tissue bands between the elephant's lung and the pleura. This is a physiological development of evolutionary adhesions, and clearly supports the pathological development of many large variations. With regard to mesenteric bands the same worker says: "In securing a proper fixation of the abdominal viscera Nature calls to her aid processes which are usually regarded as pathological." In this passage "Nature" can obviously be translated into a series of modifiable and transmissible phenomena.

After reviewing phenomena such as these, the conclusion seems inevitable that single small favourable variations have not done the whole work of evolution. They may play their part as correlated changes; but they then take their place in a series of which the causes can be recognized. In combination with reasonable views of use and disuse, and of increased or decreased blood-supply, they may, perhaps, be held to explain such phenomena as the delicate co-aptation of some cardiac valves. Their place in the explanation of the phenomena of mimicry seems obvious. But though they may help us to comprehend how tissues become finished structures, if they are combined with the results of functional energy, they yield no hint as to great or decisive developments, and the mechanism involved in them. If the reasons adduced for the thesis laid down carry any weight, it is obvious that many, if not most, of the really decisive variations in all internal structure depended, and still depend, not on variations which can be called favourable, but on those that for the major portion of the organisms involved are directly disastrous; not on variations which are small, but on those which are big enough to be appreciable as the cause of immense functional and structural results; not on changes which can in any sense be called spontaneous, by which we may suppose are meant those no cause can be assigned to, but on variations, which, though they occurred ages ago, were obviously due to the very same causes that the pathologist can demonstrate to be working at the present day. Only such organisms as respond by direct reactions in a manner that finally turns out to be useful, or at the very least compatible with life and reproduction, are able to survive. The whole of growth and development thus becomes largely a function of effective morphogenetic repair to organic failure and disease.

Though this is not the place to deal at length with the vexed question of transmission of modifications, it may be remarked that the foregoing arguments seem to imply that such alterations, as a matter of fact, are inherited. I think some progress can be made if we simply assume provisionally that organisms *do* tend to repeat themselves, and that it is *unlikeness* rather than *likeness* which requires explanation. We know that gross unlikeness is almost always due to a lack or excess of some internal secretion, hormone, or enzyme, and from this it may be inferred that likeness is due to such catalytic machinery coming over in the zygote, and to each differentiation producing anew its own peculiar products which stimulate or inhibit further growth and differentiation. Some time ago I was struck by a remark of Starling's, that each new organism seemed a fresh "creation." He gave this up on account of the difficulty he found in the "time element" of the problem; but I venture to think he was right in his surmise.[1] There is a growing body of opinion in support of this view, as the names of Cunningham, MacBride, Dendy, and Bourne seem to bear witness. We must certainly take into account these hormonic regulators of metabolism, and if we accept the view that hyperthyroidism is the direct cause of the phenomena seen in

[1] See Chapter VII., *Heredity and Environment.*

Graves' Disease, just as hyper- or hypo-pituitarism causes giantism or infantilism in children, while a later overgrowth of the gland causes acromegaly, I see no difficulty in accepting the hypothesis that growth is determined, *i.e.* stimulated or finally inhibited, by non-living catalysts or secretions not necessarily confined to the endocrine organs. In this way a bridge may perhaps be built between the orthodox Weismannian and the Lamarckian. Growth and character *are* caused by determinants; but these are not parts of the cytoplasm itself, they are the machinery by and through which living matter acts. The organism is not built up by special protoplasm, or by entelechies, or by any mysterious *élan creatif*. It arises from the definite influence of definite catalysts originating, in an orderly sequence, as the organs become differentiated, while the individual is as a whole exposed in an infinite progression to the internal and external stimuli of a like but slowly changing environment to which it reacts. The factors which did the work are working now.

To recapitulate the tentative conclusions arrived at, it may be suggested that:

1. Mechanical reaction to stress is a general law of all tissues.

2. Morbid conditions in many cases give rise to repair which becomes physiological.

3. Such repairs lead to new functions, new stresses, further morbid states, and further repair.

4. These factors are some of the main causes of specific and generic differences.

5. In all probability transmission of changes caused in the way indicated takes place by a morphogenetic reply *in utero* to increased functional stresses.

6. As it is a narrow view to assume that pathology

in all cases tends to death, the study of pathology and general physiology should be part of the preparation of the biologist.

REFERENCES.

BAYLISS, W. M.—" Principles of General Physiology," 1918.

BLAND-SUTTON, Sir JOHN.—(1) "Tumours," 1918; (2) "Evolution and Disease."

BRAMANN.—" Descensus Testiculorum," *His. Arch.*, 1884, pp. 310–40.

HUNTER, JOHN.—" Works" (ed. Owen), vol. iv., " Descent of Testis," p. 1.

KAMMERER. P.—*v.* MacBride's paper, *infra.*

KEITH, ARTHUR.—" Nature of Peritoneal Adhesions," *Lancet*, London, 1914, p. 362; " Menders of the Maimed," 1919.

LEWIS, T.—" Mechanism of the Heart Beat," 1911.

MACBRIDE, E. W.—" Are Acquired Characters Inherited ? " *Trans. S.E. Union of Scientific Societies*, 1917.

STARLING, E. H.—(1) " Physiology" ; (2) " Law of the Heart," 1916.

SUTTON, H. G.—" Lectures on Pathology."

WOLFF, JULIUS.—" Law of Bone Transformation," Acad. Science, Berlin, 1892.

CHAPTER IV

Inhibition and the Cardiac Vagus[1]

THE history of science records the birth, life, and death of many conceptions, which, although they seemed to reconcile contradictions triumphantly, in the end did but serve to show how hypotheses can rise to the rank of theories, and yet finally be disposed of by some stray fact. Just as some conqueror in his hour of victory falls to a chance bullet, so they succumb at last. They may for the time be "true"; they serve, that is, as a temporary shelter or clearing house for contradictory observations, and thus produce the semblance of order. Deep within them there may be even some hint of real explanation. It was so with the ancient view of the arteries, to which the very word bears witness. They were, indeed, air vessels even if the observations leading to such a conclusion were

[1] An abstract of this paper was printed in *The British Medical Journal*, September 14, 1918, and since re-writing and developing it, I note that R. M. M'Nair Wilson ("Meaning of Tachycardia," *ibid.*, January 17, 1920) practically adopts Luciani's view, with which I was not acquainted, that the vagus is "a nerve of diastole," and adds, "this nerve would seem not to be inhibitor in the narrow sense, but rather to act by increasing the filling time in response, no doubt, to stimulation from the cortex." Although I do not wholly agree with this in all its implications, yet as I have expressed doubts as to whether tachycardia is to be attributed to disturbance of the pace-maker itself, it is of interest to me to note that Wilson holds the view that it is due to a compensating acceleration when the ventricular systole cannot get rid of the diastolic intake. This, I imagine, is also the opinion of T. Lewis, who states that tachycardia does not originate in the pace-maker.

incomplete. So, too, with Phlogiston, which was both false and true, and passed away as explanation when the actual nature of combustion was discovered. Many such conceptions must exist now, and possibly among them may be reckoned some orthodox conceptions of physiology.

It is a mark of false but useful theory that in the end it raises more questions than it disposes of. In its explanation other hypotheses are built up which contradict each other, while none can be found to reconcile them. But if the original conception is held obstinately, even further observations are tortured into supporting it. In such cases free speculation and criticism may play a useful part.

If there is any physiological subject in which speculation of all kinds is allowable on such grounds it is, perhaps, Inhibition. There are facts of observation in abundance, as any text-book of physiology proves, while the hypotheses, sometimes misnamed theories, which seek to explain them are both difficult and contradictory. It seemed to me, when considering the subject, that the function of the cardiac vagus, in which, according to accepted views, inhibition means not only cessation of action in some muscles, but the actual weakening of the whole heart, might form a key, if not to unlock the mysteries of inhibition, at the least to show some possible flaws in accepted opinion. It appeared likely that not only had results obtained in the laboratory, often of a pathological or traumatic origin, not been compared with normal physiological action, but that few inquirers had sought for analogies in other organisms by which inhibition and its action could be understood. It certainly appeared as if some clue were needed to the problem, so far without real explanation, as to the manner in which the vagus centre could "put the heart out of action," and yet increase the action of the intestine. In saying so it has not been forgotten that inhibition is admitted by most to be no more than a covering word for various observations. Such a covering word, however, tends to obtain illegitimate sanctions by continued use, and if it is asked to shelter not only mere physiological stoppage of action, as when a muscle is thrown out of gear, but also a pathological process in which the subject may die of cardiac failure, a better one must surely be sought.

In the first place, it may be asked whether inhibition, in any case, is a safe word to employ, even if the facts observed are found to support the general notion. Nothing is clearer than that the use of a word, which is, as it were, sanctified by special employment in other connections than those of science, needs the closest examination. The connotation of the term as commonly employed is purely "psychological," that is to say, it is a "portmanteau" word for the interruptions of functions by "forbiddance," or obvious and useful shorthand for a complex of conditioned reflexes which, on being excited, repress certain actions by turning energy in other directions. For we cannot suppose that an "inhibited" clergyman does not in some way employ his energy on paths previously little used or not used at all. Just as spoken words are sound signals which excite reflex action, so the bishop's written words of inhibition, when they stop a certain function, set others in action, whether it be over emotional tracts of indignation, surprise, or anger, or over carefully considered remonstrance worked out by "volition" over the pyramidal tract in motor reactions which produce an answer and demand investigation. Yet using the common verbal shorthand, we say the clergyman has been "inhibited" by the bishop, as if some direct influence, not to be analysed into historic ecclesiastical tradition and custom, has produced mere inability, and reduced the inhibited to temporary paralysis. Taking the word into science has produced similar results since some eminent physiologists have used the very word "influence" of the supposed vagus action on the heart. They would doubtless own that they, too, employed the word as shorthand; but if, as I think the facts show, all sorts of inhibition, save those due to the pathological conditions vaguely summed up as "shock," are the result of substituted excitational actions, direct or upon other paths, there is no more need to use the word as opposed to excitation than there is to say that work is inhibited by the sound of the dinner-bell. What really happens in such a case is the conditioned reflex-closing of some synapses of the brain, and the easy opening of others leading to the reflex instinctive satisfactions of food.

The answer to these questions seems to depend upon the processes which take place in one muscle when its antagonist is stimulated. There are probably few more difficult subjects in physiology than that of contractile tissue; but to say that when a muscle goes out of action it is "inhibited," surely takes us no further, since it is only putting into obscure words what we already know. It was the belief that the action of the stimulated cardiac vagus was excitatory of some really active process which led me to inquire whether inhibition in the sense of weakening ever occurred save in pathological cases, for, if it did not, it seemed to follow inevitably that the relaxation of a muscle was in some way a positive process in which there was some substituted action, not a time of rest. For a muscle's time of rest should be the refractory period. At first it seemed as if the facts could be explained, if not by a process of "drainage," at least by the deduction that a muscle "went out of action" owing to the fact that it sent no afferent, and received no answering efferent messages. Yet as I believed that there was a definitely active process shown in the diastole on normal physiological stimulation, it at last seemed certain that muscle-lengthening was not at all an inhibitory but an exciting process. It did not seem legitimate to say that lengthening took place "naturally," even if the word was interpreted as implying local processes of communicated strains, and cessation of strains, acting directly as kinds of primitive reflexes. What then is the action of muscle when lengthening? As there are deformations of the muscle cells in contraction, why should there not be active deformation in its opposite? Such deformations must depend on physico-chemical factors such as surface tension and osmosis. If a muscle cell alters its shape when stimulated to contract, we can imagine its molecules ranged in a column of two, "forming fours," and closing up. In the opposite process the column of two molecules will become a column of single ones. Thus relaxation becomes a positive lengthening process of active deformation. If there are no special muscles of diastole in the heart acted on directly by the vagus as a motor nerve, some such process must account for the active diastole. It was a long time after some of these considerations occurred to me that a real explanation of cardiac negative pressure seemed possible.

We find it, however, stated authoritatively that stimulation of the cardiac vagus is followed, not by the mere throwing out of action of another muscle, but by a slowing of the rate, a diminution of the energy, and a slackening in the rate of conduction of the Bundle of His and of the whole heart. The vagus fibres are thus conceived as actually depressant fibres, while the accelerator (or augmentor) acts, it is said, in exactly the opposite way. Since such views are founded on observations first made by the Webers, which cannot be placed in the category of ordinary reflex inhibition with substituted action, some further examination of the deductions drawn may surely be made with a view of determining whether the cardiac vagus really plays the part assigned to it, and whether the view stated above can throw light not only upon ordinary "inhibition," but on the real nature of the cardiac diastole.

Although there is great reluctance on the part of physiologists to acknowledge that experiments, however great their value, are often misleading, it is just as true to say so as to say that what happens *in vitro* is not always repeated *in vivo*. If it were, practical medicine would be less uncertain than it is. But just as the living organism with its unmeasured complexities thwarts and disappoints both physician and pharmacologist, so the animal experimented on, in conditions which are pathological and unnatural from the very beginning, cannot always show, and cannot be expected to show, the reactions due to natural stimuli when subjected, probably under conditions of trauma, to stimuli with which evolution has not made it acquainted. To say so much is not to urge any vital objection to experiment, but merely to caution those who, when they obtain interesting results, believe they are physiological. If in general a negative effect obtained by nerve stimulation requires explanation, how much more is needed to make it credible that evolution has contrived, by the mechanism of the cardiac vagus, a means not only of weakening the organism, but of actually bringing about its death ?

When any man dies of sudden heart failure without cardiac disease or degeneration it is commonly attributed to "shock." Whatever "shock" may be, and something relevant to the subject may be urged later, it is usually brought about by violent stimulation of an unusual kind, such as severe trauma with its concomitant results. It is obvious that the heart, especially in delicately balanced organisms, is subject to continual fluctuations in its rate and energy. In it any excitatory, or depressant, stimuli bring about reflex cardiac results, in which, doubtless, the vagus is often implicated. In a minor degree they, too, may often be said to suffer from shock, for in that condition the state of the circulation is a major factor. If they are depressed there is weakening and slowing of the heart, a possible accumulation of blood in the abdominal veins, or even some loss of the fluid constituents of the blood by failure of osmotic balance, such as seen in undoubted traumatic shock (Bayliss), temporary anæmia of the brain, collapse, and possibly loss of consciousness. Such results, although they may upon occasion save the life of those who suffer from them, are undoubtedly dangerous, and in that sense pathological. It seems possible, then, that the equivalent or allied phenomena seen in animals, when the cut vagus is stimulated artificially, may be of a similar kind, and that any physiological deductions as to the action of the cardiac vagus are, to say the least, somewhat hasty.

Since shock has been of late the subject of much research, and of especially valuable practical work by Bayliss, something at least is known of its nature. Judging, however, from recent notices of it in medical journals, it is to be observed that the main symptoms of severe, or possibly lethal, shock only are dilated upon. The influence of damaged muscle products, and the loss of blood fluid through the walls of the veins and arteries, are certainly, as it seems, the principal phenomena of the drama; but since all physiological phenomena can only be conceived as verging gradually into pathological forms, and as we cannot understand pathology, except as divergence from useful function or structure, it inevitably follows that stimulation itself can be so accentuated that it becomes, firstly, excessive, secondly, abnormal, and at last crosses the vague border-line and becomes pathological. So, from the psychological, or complex cerebral point of view, we see the varying results of mild and severe surprise, mere fright, or excessive terror exciting more and more violent motor reactions or producing collapse, paralysis of all effort, total unconsciousness, or even death. There is therefore no need whatever to confine the use of the word shock to extreme phenomena. Anything which interrupts normal function, whether by vaso-motor means, by the excitation of some glands, or by producing synaptic block, or its exact opposite, may be ranked under its heading.

In accordance with what is laid down elsewhere, something more may be learnt of these phenomena if any mechanical, biological, or social analogies can be discovered. Incidentally such an inquiry should throw some light, however dim, upon inhibition itself. When a social "shock," such as a great national calamity, is experienced, what are the phenomena observed? The outstanding fact is that every one's attention is diverted from his task, and that for a time, longer or shorter according to circumstances, work ceases, or is greatly slackened. In certain factories, for instance, in which the energy used is supplied by machinery, the engineer might even stop it altogether. It may be remarked, by the way, that in such a case energy would be stored in the boilers if the fires were not drawn or neglected. Such a storage of energy may be seen during temporary slowing of a physiological process, such as at first follows on vagal stimulation of the intestine. If the social shock is not too great, something of the same result follows. If the work in hand is very necessary, it is probably returned to with greater vigour after the pause. Or it is diverted to functions still more necessary in the new circumstances. But when such a shock is very great, it is not followed by physiological renewed action, nor is it diverted. Action is entirely "inhibited," and energy is wasted. How is that energy wasted? Energy must do work. What then is done? It was said by a great physician that unity was health, and separation disease. He spoke truly, for with separation in any organism there is waste of energy. Nothing gets done. In a social organism there follows on great shock a degree of disintegration, with concomitant anger, argument, recrimination, so that energy is wasted in mere social heat instead of used in combined directed labour. Shock, then, is plainly a disruptive phenomenon, whether seen in a social or animal unit. In the animal no organ works well and none works with another. Secretions, hormones, catalysts, and the whole machinery of life are altered. The nervous system ceases to function rightly; tone, nervous or muscular, is lowered, the blood accumulates in the splanchic area, the veins lose their serum. Instead of real symbiosis there is a dead indifference—if the psychological phrase may be allowed: the fundamental hostility at the bottom of symbiosis may cease its powerful action. It is almost as if in a wrecked ship all hands broke into the spirit room, for in the shocked organism excretion is interfered with, and there is no cell unpoisoned, unintoxicated. Such effects of wasted, or undirected, energy may be seen in special cases to which the word shock cannot be employed. In the failing heart auricular fibrillation is the untimed contractions of disorganized fibres. It is the same with cardiac flutter. In a racing boat, when exhaustion overtakes the crew, they do not pull together. Unable any longer to receive the rhythmic stimulus of stroke, each man's reaction time or personal equation masters him. And each man's differs. The boat slows and, perhaps, finally stops. "The crew went to pieces." It is so with utter exhaustion. It is so in shock. And undoubtedly the liability to such complete disruption increases as the organism becomes higher, and the degree of interdependence of the organs increases. In the most developed nervous types the heart seems the first organ to feel shock of any kind. Something or another has "gone to pieces"; some function has been interrupted, whether by a disintegrative process in cells leading to an interruption of reversible reactions of colloids, or by some other failure. The processes by which energy is stored and released in muscle are as obscure as they are remarkable, and it is possible that cardiac shock may be found at last to be the result of an abnormal colloidal process depending upon an excessive "trigger action" of the cardiac vagus.

For many reasons it seems impossible to accept unreservedly the physiological doctrine that stimulation of a nerve is but "trigger action" to the muscle it sets going, and that no more energy passes over from the end-plate than an amount so small as not to be measurable. If some of the conclusions as to nervous action were not called in question by other accepted, or partially accepted, views, it might seem hazardous to make such an assertion.

But assuredly Wrightson's *Theory of Hearing*, which has able advocates, throws doubt on many accepted opinions. Moreover, those who have had the doubtful advantage of receiving a severe and unexpected electric shock with a powerful muscular reaction which, as it seems, might in some cases tear away a ligament or even snap a bone, will have suspicions of the " trigger " doctrine. If the function of the vagus is not to produce some kind of excitation, the phrase " trigger action " does not apply. " Trigger action " in a gun is a measurable amount of energy, and so is the energy in the cartridge. And in ordinary cases, where the gun and the cartridge are what we may call " physiological," that is, in such a state that the normal hammer fall produces the normal explosion, an abnormally powerful hammer impact will produce no more powder energy than a merely adequate one. Yet if a powerful nerve excitation occurs, there is more than a normal explosion of muscle energy. In this case, if the nerve acts as a whole, more energy does " go over," for, if in ordinary conditions only a few fibres are affected, with the stronger stimulation all may be called into play. In either case the facts suggest that more energetic stimulation does cause greater, or even disastrous effects, showing that " trigger action " is not a sound analogy. It is therefore easy to understand how it is that abnormal vagal stimulation results in " shock " or disintegrating action. There seem more analogies between electric and nervous phenomena than the usual nerve theories allow. For if in electric " flex," composed of a large number of very fine wires, some of these fibres are cut, the lamp does not light, whereas, if more volts are applied to the unaltered wire, " shock " results, and the incandescent filaments fuse. It should be remembered that action resulting in shock is not an entity, *totus, teres* *atque rotundus*, it is a function of the two variables—stimulus, and the condition of the stimulated organism. It may be little or more and more. The further it is considered the more justifiable it appears to regard it as a stimulus producing pathological effects, and such views are in keeping with the notion that the vagus normally is not "depressor," but, being a most delicate adjustment agent, can easily become such. We can certainly imagine that its action is depressing if all the vagus fibres are excited at once, which probably never happens in physiological conditions. So very rarely can all the fibres of the biceps be excited together. There are nearly a thousand fibres in its trunk. If the violent tonic spasm, produced by strong electric stimulation, excites them all together the phenomenon is explicable. Otherwise we must assume, as said above, what no physiologist seems to believe, that energy does really pass over from the end-plate into the muscle itself.

If then shock be a complex of such phenomena, and if it is hard to conceive that evolution has made physiological stimulation of the vagus a means of destruction, it necessarily follows that its experimental stimulation, leading to weakening and failure of the heart, is essentially pathological, and that from what occurs no legitimate physiological deductions can be drawn. The Webers discovered an interesting fact; but all they noted must be classed as pathological or traumatic. Doubt is thus thrown upon the view that the vagus can weaken a muscle in one place and stimulate it in another. Nothing will co-ordinate the facts but some proof, or suggestion of proof, that the vagus exerts both on heart muscle and smooth muscle an "influence" which helps both to function better.

What then is the real function of the cardiac vagus?

When it is considered that every act of breathing and every change of posture send vagus messages to the heart, it seems obvious that the nerve fibres are adapted to control the heart's action, and enable it to do its work. Such messages are truly physiological, and cannot be measured in electrical language. But though they cannot be so measured, the changes induced are of definite advantage to the organism, and indicate one of the most delicate adaptations to gravity, or slight efforts, to be found in the mammalian body. Vagal or youthful irregularity of the heart is doubtless of a similar kind. In this kind of cardiac arrhythmia, still frequently mistaken by some medical men for serious disease, the heart slows after every inspiration, while, if the patient holds his breath, the irregularity tends to disappear. Though it is now known, owing to the work of Mackenzie, that it has no pathological significance whatsoever, it certainly has a physiological signification as showing to what immeasurably small stimuli the normal heart can and does respond. It may eventually be found that, though the slowing occurs during expiration, the vagal stimulation is experienced during inspiration, when the lungs expand. In a pathological case, the mere act of swallowing stimulates the vagus, and produces heart-block a few seconds later. The more these facts are considered the less likely does it seem that a tied-up animal, with a cut vagus electrically stimulated, can be held to show phenomena on a cardiogram which are remotely relevant to normal vagal action. Moreover, even when pressure over the vagus is applied at the neck and slowing results, there is no evidence to prove that the result is directly due to the pressure, since it is, on these general lines, far more likely to be due to the partial interruption of the circulation, and a vagal attempt at vaso-motor readjustment.

From such considerations it follows that an inquiry must be made as to how the vagus acts on the heart physiologically, and what its real functions are. Such a question leads to a fresh study of the heart's mechanism, and the rôle played by the augmentor as well as the vagus. Even if the notion that the vagus normally weakens the heart be put aside as contradicting the whole course of evolution, and if the facts can be otherwise explained when it does happen to have that effect, it may, at least, be admitted that it certainly slows the heart on stimulation, just as the augmentor or accelerator fibres quicken it. But reasonable slowness of action by no means implies weakening or fatigue. The heart can be slowed in many ways, by the toxins of fatigue or disease, by high blood-pressure, by a depletion of the higher centres owing to vaso-motor action (Mackenzie); but if it is slowed physiologically, it must be for advantageous action, and what is seen in experiment is, at least, partially irrelevant, even if the whole of the phenomena, when understood, can be linked together. Following the method hitherto adopted, we may seek for illustrations of slowing in something that resembles an organism, and try to discover why it happens, and what it effects. Let such an organism or individual be a University eight. In a well-trained crew the endeavour of the trainer has been to get a long, slow, "well-pulled-through" action; but when the crew become tired they are apt to accelerate the stroke, and make it short and "snatchy." It is found that more power is obtained by the long and slow stroke, and when the coxswain or the stroke oar think it time to quicken, both are well aware the reserve power of the crew is being drawn upon. When the slower rate is maintained, "inhibition" is the inhibition of the accelerator, not inhibition of the strength of the crew. By inhibiting rapidity and the waste of unregulated energy, power is conserved. Quite legitimately we may regard stroke as the pace-maker, and cox as central control. If in the heart we regard "inhibition" by the vagus as inhibition of the accelerators themselves, we obtain a physiological view of the whole cardiac drama, which, in fact, puts it into line with the muscular phenomena of reciprocal innervation. I am aware that physiologists, dominated by what they observe in the laboratory, maintain that such an illustration does not illustrate, as they say the heart's force is really weakened. But the objections to this are many more than those already mentioned. All the facts of mechanics and physics are against it. When an engine is stopped or slowed, it is not weakened. On the contrary, it is strengthened, *i.e.* energy accumulates, and the boiler pressure rises. Questions of energetics also arise, for if, as physiologists say, the nerve only pulls the trigger, what action occurs in the heart muscle as it slows? Does it waste its energy under normal stimulation, postural, inspiratory, or expiratory? And if its energy is wasted, what becomes of it, and how does its free energy become bound energy? It seems, having got so far, that no effort has been made to ascertain whether the real function of the vagus, as regards the brain itself, is not to control the accelerator centre, and the action of the accelerator centre to modify vagus action. Such an opinion might, I think, correlate and explain many of the observed phenomena, and it would certainly bring them into line with those seen in engines of all kinds, while it does not contradict direct cardiac action.

It still remains, even if these views are allowed to have any force, to ask in what way the vagus actually influences the heart? If it slows it, not to weaken it, but to allow it time to recuperate and gather energy, just as the vagus when acting on an intestine has at first a slowing, "inhibitory" effect upon it, on what in the heart does the vagus act? Such a question leads to the consideration of the diastolic mechanism, which seems so obscure that no information whatever is to be obtained on the subject. After consulting all books within my reach, the utmost I have gathered is that the diastole is "an elastic rebound," in some way connected with the *columnæ carneæ.* On applying personally to certain authorities I was told it was "a vital process." But so is the whole of life. The answer answered nothing. We know that there is negative pressure in the heart during diastole, so the old theory of the passive diastole cannot stand. There are few, if any, elastic connective-tissue fibres in the ventricle, whatever there may be in the septum or the base. Is then the diastole after all a muscular process in the sense that certain layers contract? One of our greatest authorities tells me that there are no reasons for supposing that this is so. The whole of the muscle layers seem adapted only for the systole. It is true that in the systolic contraction there appear to be torsion strains. If we adopt the view that skeletal muscles are all systems, and that a single muscle cannot exist, it is perhaps conceivable that certain layers of cardiac muscle are stretched during the systolic torsion contraction, and that the diastolic rebound is thus muscular. Yet if the view is accepted that the mere lengthening of a muscle is a positive and active process, and a change in molecular order, which in some cases can do work, this somewhat unlikely hypothesis can be dispensed with. We have merely to inquire why in ordinary cases a lengthened muscle is said to be relaxed, and why the positive cardiac diastole, capable of producing a negative pressure, which according to Stefani is increased by vagal excitations, must obviously in some sense be unrelaxed. Because an inactive voluntary muscle appears soft it by no means follows that the lengthened cells are themselves so. They have merely ceased to pull on their origin and insertion. One fibre is not attached to another, and in contraction what we observe is the general tensile strain. But in the heart this condition of loose muscle fibre does not exist. According to Schäfer cardiac fibres differ greatly from voluntary fibres: "their striations are less distinct; they have no sarcolemma; they branch and unite with neighbouring fibres, and their nuclei lie in the centre of the fibres." Of these differences, and here I follow a brilliant suggestion of Keith's, the really important one is that "they branch and unite with neighbouring fibres." If the diastolic muscle action really does work, the fibres cannot slacken and bend as in voluntary muscle. They form an actual network, an interdigitated or branched growth-mechanism, and must move together. Excitation is not transmitted from fibre to fibre in skeletal muscle, but it does so pass in cardiac as in smooth muscle, and all the cells are excited in waves. It is obvious that these millions of short columnar cells, each with its restraining connections, have to act as a body, cell with cell, fibre with fibre, and layer with layer, since the layers are so much part of each other that anatomists differ as to their number. In such a formation we have a most remarkable and unique engine, very different indeed from the fibres of voluntary muscle isolated in their sarcolemma, and a rough illustration of its mechanism may be afforded by comparing it with what we see in the instrument known as a "lazy tongs," in which interdependent, interbranched, and hinged lozenges are shortened or lengthened at the user's will, while in either state the whole tool remains more or less rigid. On receiving stimulation in a normal condition of the myocardium it is assuredly a case of " all or nothing," for a fibre cannot really move by itself, although in the pathological state of fibrillation there are useless unco-ordinated twitches. Even if I were qualified to enter into the whole question of muscular action, and most assuredly I am not, to do so would be unnecessary in this question of the diastole. It is sufficient to note that there appears a fairly general consensus of opinion that contraction is due to surface action, and that, though oxygen is needed for the energy which restores potential, combustion takes no part in the actual work done. We are here, however, not concerned with contraction, but with elongation, and, though it is difficult to understand in what way osmosis plays a part, it is known that in muscle action there is movement of water (Bayliss), and that fatigued muscle readily absorbs it. It may, perhaps, be said that during contraction the water is expelled into " lakes " in the interstices of the network of cells, that during the refractory period the oxidative processes which restore lactic acid take place, and that elongation is osmotic expansion. Thus contraction has an outflow and elongation an inflow, which, it may be suggested, puts the whole process on a par with vaso-constrictor and vaso-dilator phenomena. If this is so it seems perfectly legitimate to regard the muscle cells as complex sets of reversible hydraulic presses, and to infer that, though apparently less powerful than contraction, lengthening of muscle is a process exactly analogous to vaso-dilatation. That this process may be increased by normal stimulation of the vagus centre can hardly be doubted, and when it is said that the vaso-dilator centre is not yet discovered, though known to exist since vaso-dilator nerves leave the cranial system, it seems as if the actual facts had been obscured by hasty theories of inhibition, and that the portion of the bulb where the vagus arises is the actual centre which is still looked for. We might, then, assume that vagus action is the same as positive vaso-dilatation, however much the phenomena are obscured in the intestine or elsewhere by subsidiary controlling mechanisms, such as are probably found in Auerbach's plexus, and infer that the whole action of the heart is but specialized vaso-constriction and dilatation by an ancestral motor nerve and its later subordinate ganglia. It might even be said that the Keith-Flack node is the Auerbach plexus of the heart. If this is so, vaso-dilatation is everywhere caused by a positive elongation of a muscular ring, which pushes outwards while held in position by neighbouring tissues; the cell-lengthening being caused by osmosis. When we deal, not with such small muscular systems as an intestinal or arterial coat, but with a larger connected mass of systems such as cardiac muscle, it is not more difficult, or so it seems to me, to understand how a negative pressure comes about in the ventricle than in a pump. In fact, the heart in more senses than one is a double pump, for it not only expels blood but draws it in. On further experiment it may be found that even the auricles have a feeble aspiratory power. I find myself totally unable to credit any other view since the heart, when removed from an animal and kept in a nutrient Ringer's solution, continues to expand and contract actively. Very many years ago I was much impressed by observing the heart of a pelagic shark, of the genus Carcharias, beating in the open air of a hot tropical day. I held it in my hand, and found some pressure needed to keep it closed. On opening my fingers it followed them, and went on beating. I put it down upon the deck, and placed a coin upon it. The heart continually raised and lowered the piece of money. When it was raised the heart was widely expanded and semi-transparent. As the organ contracted to a mere knot it lost its transparency. But the point is that the detached heart, even in hot, dry air, for very many minutes actually did work. I have been assured that I may have mistaken the systole for the diastole. I do not see how this can be. Ignorant as I was of physiology, I could still observe the time of its greatest expansion when it raised the weight put upon it. In spite of our ignorance of the exact mechanism it seems impossible not to think that there are direct diastolic agents, and that it is they which are governed and regulated by the vagus. It should come into play, especially when the heart is irritable and shows a tendency to rely upon acceleration rather than the "long pulled-through stroke" permitted by an adequate diastolic action. Such action would tend to keep the blood-pressure normal, and increase the coronary blood-supply. It thus becomes easy to understand an efferent cranial nerve acting not as a simple motor nerve, but as part of the autonomic sympathetic system, and it puts vagal cardiac action into line with its positive effects on intestinal movements. It is not too much to say that the physiological stimulus affecting the vagus probably depends on an increased irritability in the *medulla,* consequent on a lessened blood-supply, which reflexly exerts its influence on the vagal centre. The whole drama of the heart, independently of its automatic action, thus depends for stress and change on the regulating effects of vagus and accelerator. The accelerator, indeed, seems a better term than Gaskell's "augmentor," for its chief rôle appears to be that of overcoming the inertia of the "pace-maker," and urging the heart to increased rapidity of action until the vagus once more controls it. So the accelerator replies to a rapid stress, the vagus to a continued one. Tachycardia as a morbid condition is probably not always due to abnormal alterations in the "pace-maker." When the organism grows weak, and the blood-pressure falls, the cerebral arteries and coronary system can only be kept going by increased rapidity, which makes up for the small volume of blood sent into the aorta at each ventricular contraction. Every one who has observed tired and worried workers, forced to continue by urgent stimuli, has seen them go through such stages. A man who works under pressure with a shovel increases his rapidity and decreases his load. Seamen tend under similar circumstances to take short ineffective pulls on the gear at which they are hauling, and are apt to do it in silence without the "pace-maker" of a rhythmic song. I may be exposing myself to the ridicule of the unobservant if I liken to vagus action the mate's voice urging them to use their strength more rhythmically and conservatively. But every worker who has toiled under stress will be able to recall analogies in his own experience which strengthen such an illustration.

It appears, then, as if the vagus and accelerator fibres had no function of very great importance in health, rest, and easy normal conditions, although without doubt they make minor corrections in the cardiac mechanism at all times. The necessity of explaining " inhibition " in the heart thus seems only to exist in the laboratory, in the casualty ward, or on the operating table. Then the conditions are pathological; the cases are cases of "shock," if shock is disruption of united organic action with concomitant effects upon the organs by which stability is assured. Such an explanation seems in accord with what is known of the

8

vaso-motor system as it responds reflexly to the needs of the somatic cells, or to emotions conditioned by adrenalin and other glandular products. But the chief point is that there is no real contradiction in such a view between the cardiac and intestinal vagus action. Both tend to increase the working power of the organ they control. Moreover, if these arguments have any weight, such is assuredly increased by the fact that the phenomena accompanying the therapeutic action of digitalis no longer contradict vagus action, but show that the drug actually assists it to work when normal control breaks down and the degenerate heart is under the influence of the accelerators, or a flurried irregular stimulus of the pace-maker, with concomitant irregular muscle fibre discharges, such as are seen in auricular fibrillation. It is true that pharmacologists assert that the action of digitalis differs from vagus influence. This is only natural since they are apt to bolt their physiology whole, as it is given them by specialists in that science. So indeed the physiologist himself, with regard to drug action, leans with too much faith on the pharmacologist. As was once remarked to me by an eminent professor, there is scarcely a drug known to medicine which would not take a lifetime to study properly. Certainly clinicians would agree. Cushny says that the inhibitory action of the vagus tends to render tone less complete, and to produce weaker contractions than digitalis. This is in accordance with orthodox opinion. But the evidence is not convincing. What *is* of weight is the result of the experiments with this particular cardiac drug. Even if ancient accepted experiments, drawn from the text-books or the practice of the physiological laboratories, are repeated, they are of no more importance than the original ones founded, as I have endeavoured to show, on unphysiological lines. To repeat, and keep on repeating, that a normal stimulus can have a direct weakening effect does not convince those desirous of examining the problem afresh. It may be recalled, perhaps, not without advantage, that many single experiments, or even dicta of authoritative ancient physicians, are as duly repeated from one text-book to another as wrong definitions in some big dictionary are copied in its successors. In any case the physiologists and pharmacologists speak not as physicians, most of whom, I imagine, are under the impression that digitalis in therapeutic doses aids vagus action, slows the pulse, obtains a better diastole directly, and allows the heart pause sufficient time to gather up its energy and increase its general tone and its hæmic output with relief to all the symptoms which called for its assistance. When the heart has been thus helped the accelerator is no longer irritated into increasing the heart-rate, the pace-maker is restored to its normal action, and once more dominates the irregular discharges of the degenerate myocardium. To say, as the students of drugs say, that the symptoms in the second, or poisonous, stage of digitalis are like vagus action, and that cardiac work is therefore less well done, is to mix true observations with false. At the least it seems to imply that digitalis then acts through and on the vagus nerve, whereas its therapeutic action suggests that what it does is to increase the working capacity of the cardiac muscle by modifying its irritability and allowing it time to recuperate, thereby permitting normal vagus action to continue. It thus assists the complex reversible reactions which enable muscle to work at all. If larger doses stop action directly, or prevent the muscle cells from being supplied with necessary proteins, or with fuel, it is in such cases that the word "inhibition" seems truly applicable, for poisonous doses of digitalis no doubt slow or stop recuperative cardiac processes as well as many others.

In such conceptions there is no mystery, and no necessity for the hypothesis that the vagus, acting as a trigger, releases some particular compound which weakens the heart. If in 1906 Sherrington used the expression "inhibition, whatever that essentially may be," it is far more likely that its nature will be discovered by resort to what we know already, than to such unevolutionary notions of "weakening." It seems that H. O. Thomas, who was not only interested in bone surgery, came to the conclusion that "inhibition is the suspension of life, not the action of special nerves." That he meant by the "suspension of life" some reflexly caused cessation of action is literally certain. He actually writes in 1883: "In proof that mechanical irritation of this nerve (vagus) induces a condition of shock, we have the accepted fact that atropine protects the nerve from the shock consequent on mechanical disturbance. I have not yet met with any evidence which proves the existence of any inhibiting nerve fibres in this or any other nerve." In these views it seems, according to Rushbrooke, that Thomas followed Joseph Lister, also of Liverpool, who wrote on the subject in 1859. Although most, if not all, modern physiologists are certain that inhibition exists, and that it is centrally caused, it seems that the doctrine cannot be looked on as established. If physiology is to make secure its final "passage to physics," which physiologists, who do not resort to vitalism, are working for, some means must surely be found to reconcile the contradictions in cardiac and intestinal vagal action. Perhaps some of the dissatisfaction with current theory can be obviated by means of a different terminology, in which the "lessened action" of reciprocal innervation, that seen in the preliminary pauses of intestinal activity, and the peculiar phenomena observed in the heart, are not classed together under one word of very doubtful connotations.

It is, of course, stated, perhaps almost with violence, that inhibition and inhibitory nerves exist, that the evidence for them is overwhelming. Certainly the observations show that action ceases at times very suddenly. Activity is cut short, and a muscle with contrary action comes into play. Bell's "muscular sense" consisted, I take it, not only in central messages, but in an infinite series of reflexes. So with Duchenne's "articular sense," the loss of which, in his opinion, gave rise to locomotor ataxia. It is barely conceivable that the paths of all such delicate reflexes are known. Since the nerve cells are not so distantly related to muscle cells, which are peculiarly conductive, it can be imagined that many muscular reflexes occur even without nerves, while few neurologists, I imagine, will be ready to declare that the whole nervous anatomy of the body is now and for ever mapped out. These fields should be explored before resorting to a rough-and-ready statement of central "inhibition" in every case of suddenly arrested action. Such arrests take place under conscious, if instinctively recognized, stimuli; but since the constant course of evolution is devolution and "short-circuiting," following the laws of energetics, the naturally simpler view should be taken. If so, in every case of inhibition some short-circuiting reflex should be looked for, if the mere cessation of afferent sensory messages will not account for the phenomena. It seems as if inhibition had become a physiological Mesopotamia—a very comforting word. Certainly mystery after mystery has been crammed into it, and once established as "explanation" the endeavour is to explain it, with what results the text-books show. In things "psychological" there are few physiologists who have not welcomed Pavlov's "conditioned reflexes." By their considered use mysterious and misleading words, with a hundred different meanings, may be avoided. "Consciousness" itself, that Pandora's box of scientific, no less than metaphysical, disaster, at last gives way, and discloses itself as reflex adaptational machinery. It is time that the word inhibition should yield to the same key, for positive reflexes, however conditioned and complex, which should be capable of resolution into physical reactions, obviously rule living action of all kinds. Certainly in physiology we have not yet reached ultimate postulates or axioms, and no hypothesis should include definite contradictions, especially when we have to say "whatever it essentially may be."

In looking for explanation the human "mind" searches for rest: the brain seeks automatically for the shorter paths of cerebral activity that we call generalizations. There is something profoundly satisfying in such processes, and they can certainly be ranged under the laws of energetics. The brain has less work to do, and a complex series of opening and closing synapses is freed from continual irritation. The flood of "thought," or energy, has found a short direct channel, while all other possible paths are cut out. They are, in fact, "inhibited"; synapses close; energy does not act that way. Such a view by no means implies an acceptance of M'Dougall's "drainage" theory of inhibition, although something can be said for it. All it means is that there is substituted action. It is probably so in every case of inhibition. On a previous page I sought for some homely illustrations and analogies for cardiac vagal action. These can also be found for the phenomena in which lessened action takes place in other muscles than those of the heart.

There are "organisms" of the social kind in continuous activity, with no complete pause, not even such a pause as that of the heart. Such an organism is a ship at sea. When one watch comes on deck the other goes off. From the time of leaving one port to reaching another this continues. There is no moment at which part of the crew is not in "tone," ready for action, or in actual work. A ship has been evolved; it has grown up, it has its reactions, which we call sea customs. Although the two "watches" or parts of the crew are separate, and usually possessed of a certain jealousy or "hostility" to each other, they are connected by innumerable bonds of habit and custom. When one goes on deck in fine weather the other is "inhibited." Does such inhibition arise centrally? Although it may do so in some cases it is mostly a pure reflex phenomenon. One activity replaces the other automatically. It seems to me that such a case supplies more than a hint for what is known as "reciprocal innervation." When two muscles have been evolved together, fulfilling opposite functions, it is impossible not to imagine definite relations and connections between them. The very activity of one implies the inactivity of the other. And the actions of decerebrate animals prove it. That the central system in a ship ensures definite direction of the whole ship we know, but in the lower functions excitations and inhibitions proceed automatically, and it is of physiological interest to note that overmuch central interference with such functions produces symptoms which rapidly tend to become pathological. It is well understood at sea that "central" interference is only justifiable in abnormal conditions. Thus so long as the crew function normally no officer enters the fo'c'sle except on stated occasions of inspection. If he did it would be greatly resented. Perpetual unevolutionary stimuli produce mutinies at sea, revolutions and anarchy on shore.

Another illustration of real " inhibition " will be familiar to all physicians and surgeons. A nervous patient visits, say, a urologist, and finds that his cystic reflexes are temporarily paralysed. Their action is partly reflex, partly " volitional," or under central control. The sphincter and cystic muscles are antagonistic. The surgeon observes the condition of the patient and, whether he knows it or not, a little thought must make him aware that the patient's whole field of reactive consciousness is occupied by a conviction of inability. To encourage him vocally would be worse than useless, since the less the patient thinks the better. So he turns his back, which is already a help, and sets a tap running. Relieved from the reactions caused by observation, the patient's volitional tracts are freed while the running water sets up a series of conditioned reflexes which relax the sphincter and permit the cystic muscles to act. So between stimulus and inhibition there is a long series of substituted actions. Such a conclusion is greatly reinforced by the possible opposite effect of the surgeon's action. If the patient knows the trick that is being played upon him there may be increased inhibition, which means that his energy is turned into directions which do not help, but actually hinder, the operation desired. In every case it will be found that substituted action takes place, and that no inhibition is direct.

It will probably be said that such simple illustrations have no real relevance to such an obscure subject as inhibition. Whether this is true or not the fact remains that this paper is only meant to be suggestively critical of views which do not seem on the face of them to be sound. Students, even the acutest, brought up in the light, or shadow, of certain doctrines, are naturally apt to find on every hand confirmation of the accepted. This is not so only in theology. It is a common human weakness. The justification of criticism lies in the acknowledged confusion of the subject, due, almost certainly, to inadequate definitions and the confounding of several subjects under a heading of the very dimmest connotation. It seems that we cannot use the word rightfully even as a temporary "explanation" of vagal action, although we may employ it properly in the interruption of conditioned reflexes where we get cases of substituted action which are as clearly seen on examination as the surprising physical results of apomorphine on a hystoidal patient. To believe that stimulation, or excitation of a nerve, dissipates energy, for that is what weakening means, is impossible. It has been said that the process is no more than the interruption of the storing of energy. How this can occur physiologically is hard to see. It is a mere assumption, and in any case it would not be "weakening." Regulation is not done by enfeeblement in any class of organism except in pathological states. That the heart is always being regulated even in easy unstressed conditions is obvious. But I have discovered that it is more regulated than is generally known. Judging from cardiograms of all known kinds it would be said that healthy heart-beats were of equal force. This may be so practically, but it is not so actually. By means of a liquid column in a small tube, actuated directly from the radial artery, and thrown upon a screen with a magnification of the moving liquid until it is, say, ten feet long, it can be seen that few successive beats are equal, and that the dicrotic notch, whether great or small, is perpetually varying. All muscle fibres do not do equal work at all times. Interdependence and regulation are the ruling factors of all functions, and we may expect them in every nervous mechanism. What occurs abnormally is often the roughest guide to the normal. But, if it is taken as normal, physiology pays the penalty in confusion, even though it should always regard pathology as its nearest relative—an erring sister.

When it is remembered that no science can explain itself, and that knowledge is a patterned web woven out of all the sciences, it also seems that biology, and the whole course of evolution, might be more frequently referred to in physiological work than is usual. It was suggested above that reciprocal innervation may thus be looked on as a biological evolutionary process, a case in which muscles grew up together as interdependent organs in which alternate actions, however now regulated, were a *sine qua non* of their existence. Knowing as we do the moulding effects of stresses on bone, and the facts that the scleroblasts of sponges settle and work about non-vibrating points, biology and physics may work together even in such problems, and suggest that quasi-nervous effects may be produced in muscle, not only by alternate stretching and contraction, but also by stresses communicated through and by bone and other cells. Thus, quite independent of so-called inhibitory fibres, a sudden powerful contraction of one muscle might throw another out of action by giving such intensely sensitive cells a signal of positive relaxation. It is not so long ago that the rhythmic action of opposed muscles was supposed to be due to direct innervation, not to alternate reflex contraction and stretching. It seems at times as if more was attributed to nervous action than is actually due to it.

It has been objected to some of these views that excitation and inhibition are simple concepts, even if we say with Sherrington, " whatever they essentially may be," and that they are opposed just as warming and cooling may be. But what happens as regards energy when water cools is known, as it is in the rise and fall of electric potential. What happens to muscle energy in " inhibition " we do not know, if the usual views of cardiac weakening and the depressing functions of the vagus are held. A complex chemical process at the end-plate must be posited, and of that there seems no evidence. To call in the nervous system to produce weakening toxins or the like, even by intensifying cellular catabolism, appears too great an assumption to make when there is a much simpler explanation at hand. Many of the cardiograms shown as proofs of cardiac inhibition seem wrongly interpreted. It is, perhaps, not too presumptuous to say that their interpretation is an extremely difficult art, and that even among the most expert cardiologists there are at times great differences in opinion. Having worked for months on the subject with a disciple of our greatest cardiologist I shall, perhaps, be pardoned for suggesting that the most eminent physiologists may err in such a special branch of learning. I have seen cardiograms showing the " weakening " of the heart in which the amplitude of the beats was distinctly increased, and the interval between them lengthened when the theory of inhibition required pathological slowing and decrease of amplitude. Such results undoubtedly occur in extreme vagal stimulation; but this is only what would be expected on the views expressed in this place. But to get better systoles and diastoles is not weakening. It is also said that under vagal stimulation the conducting power of the Bundle of His is "impaired." The evidence of this is merely that the heart is generally slower. Such a statement is a mere re-statement of an assertion. That the heart is sometimes unexcitable by a direct stimulus when under vagal stimulation is just what would be expected if the vagus is a controller and regulator until it conveys a shock. So to say that the accelerator improves conduction means no more than to say the whole heart is stimulated. It is, again, a mere re-statement of an observation. The truth seems to be that the accepted doctrine of the heart is full of verbal logical fallacies. To declare that "inhibition" causes lessened action is only to say that lessened action occurs because it is somehow not so strong in action as it was. We observe it, but to attribute it to "inhibition" is to repeat the error of the bacteriologists who attribute agglutinations to mystic "agglutinins," and make an observation into a cause. If it is said that by inhibition a cause is not meant, but that the word enables us to link phenomena together, the question at once arises whether they are not falsely linked. We cannot truly oppose it to excitation, which after all is one of the purest examples of a "cause" in physiology, for, whether its nature is understood or not, we can bring an infinite number of legitimate analogies to illustrate it. And, biologically, it is seen that in any organism real inhibition is effected by means of secretions which are very definite agents. According to P. F. Herring, feeding rats with thyroid causes not only forced positive changes, such as a three-fold enlargement of the heart, and doubled weight of adrenals, but negative ones such as a smaller thyroid. The gland is not needed, is thrown out of action, and "inhibited" by definite loss of function. No analogies can help out "inhibition" if in all cessations or stoppages or weakening of action we find substituted processes of direct action by definite agents. When it is said, as has been said to me, that a pure analogy of true inhibition is when a labourer drops his tools on hearing the dinner-bell, it may be answered that here we have a direct conditioned reflex overcoming excitation which has been slackened in effect by real exhaustion of energy. A true analogy for such "inhibitions" as are seen in intestinal action, even though little will be known of that until the Auerbach and Meissner plexuses are fully understood, is a labourer pausing, taking in oxygen, and gathering up energy for a big task. For on stimulating the intestinal vagus the graphs show a preliminary slowing, and then ampler, more effective movements.

That the inhibition theory is held firmly, even obstinately, is no proof of its truth. Phlogiston was satisfactory to many, and so apparently was "bad air" as a cause of malaria. And yet Varro[1] in 36 B.C. actually attributed the disease to minute animals. Columella even spoke of mosquitoes as "armed with dangerous stings," *animalia infestis aculeis armata.* It was necessary to wait nearly two thousand years to get this verified by Ross and Manson. It may be recollected that hundreds of years ago an English physician was practically ruined by attributing many diseases to invisibly small living agents. Humoralists and solidists would be alike against him. Von Uexküll said that the object of science was not truth but order, not having reached the pragmatic conception that real order is truth; but it does not seem that the theories of inhibition have provided either.

REFERENCES.

BAYLISS, W. M.—(1) "Principles of General Physiology," 1915; (2) "Cardiac Vagus," *Brit. Med. Journ.*, London, Sept. 28, 1918.

[1] See Appendix C, "Varro."

BELL, CHARLES.—"The Hand," 1883.

COLUMELLA.—Vol. i. p. 5.

CUSHNY, A. R.—"Text-Book of Pharmacology."

DUCHENNE, G. B. A.—"Physiologie des Mouvements," 1867.

HERRING, P. F.—"Thyroid Feeding of Rats," *Quar. Journ. Exp. Physiol.*, London, 1917, vol. xi.

LISTER, JOSEPH.—*Proc. Royal Soc.*, 1859, pp. 367–80.

LUCIANI, L.—"Human Physiology," 1911.

M'DOUGALL, W.—"Physiological Psychology."

MACKENZIE, Sir J.—"Diagnosis and Treatment of Heart Affections," 1916.

ROBERTS, MORLEY.—"The Function of the Cardiac Vagus," *Brit. Med. Journ.*, London, Sept. 14, 1918.

SCHÄFER, E. A.—"Essentials of Histology."

STEFANI.—*v.* Luciani, *supra*, "Mechanism of the Heart."

THOMAS, H. O.—"Nerve Inhibition," 1883.

VARRO, MARCUS TERENTIUS, 116–27 B.C.—"De Re Rustica," Book I. cap. xii.

WEBER, E.—"Muskelbewegung," 1846.

WILSON, R. M. M'NAIR.—"Tachycardia," *Brit. Med. Journ.*, London, Jan. 17, 1920.

CHAPTER V

The Theory of Immunity[1]

WHEN the late Sir William Osler jested at the expense of the new school of theorizing biologists who strive to perpetuate in peculiar neologisms highly doubtful microscopic observations, he drew a caricature of their specialized language, which had considerable merit. But while he fought against specialism run mad in verbal constructions, which at once simulate knowledge and obscure it, he might just as well have taken his examples from the armoury of certain bacteriologists. If, in the phenomena of mitosis, "the idiosphærosome differentiates into an idiocrytosome and an idiocalyptosome, both surrounded by the idiosphærotheca," such a passage is assuredly not more curious to observe than many which cram a hundred Greek and Latin derivatives and hybrids into a bacteriological "explanation." Although any science seems liable to fall into the practical fallacy of thinking a thing can be explained without reference or relation to others, bacteriology appears least immune to this logical disorder of thought. But since explanation consists in the classification of observations with regard

[1] (For abstract of this paper, and for Professor Benjamin Moore's comments, see *British Medical Journal*, December 8, 1917, and December 22, 1917.)

to more widely operating causes, or, as Mach says, in showing that any given phenomenon is the unequivocal function of its variables, what has to be demonstrated is that those variables belong to recognized classes of factors. We cannot explain life by saying that it is living, or a bacteriologist by asserting that he is bacteriological.

It is due to such habits of thought, which now appear to be confirmed, that there are fewer subjects coming under the heads of general physiology and biochemistry in a greater state of confusion than "immunity." This result may, perhaps, be due to a false belief in the profundity of the Teutonic mind which, as Benjamin Moore pointed out, when commenting on an original communication of mine in the *British Medical Journal*, had seized upon French work, and fogged it in its best later manner. It is indeed to Weismann and Ehrlich that the worst results can be directly attributed, for just as the biologist explained heredity by saying that it happened owing to the nature of the organism, that is, to its ids, determinants, and biophors, and the like, so Ehrlich in his side-chain theory invented a marvellous verbal machinery of immunization, every word of which contained a "circulus in definiendo." To the practical English worker, who usually distrusts the theoretic intellect, as if a general idea were a proof of original sin unless it comes to him from abroad, this scheme was a godsend. It appeared to save thought, and instead of examining it critically he has patched it up with new words as they seemed to be wanted, just as the Ptolemaic astronomy added eccentric to eccentric and epicycle to epicycle in order to represent the planetary motions. And yet the whole theory is obviously false. To this every modern physiologist would subscribe, seeing that it depends for its validity on the supposition that life itself is the result of giant molecules. It is, of course, wrong to infer that particular statements are not true if rightly interpreted. "Antigens" certainly produce specific "antibodies" as definite responses to the chemical nature of the "antigen"; but there is no reason why this should not have been expressed in terms implying that any foreign or hostile elements introduced into an organism tended to produce definite reactions of a protective nature. To invent or to lead to the invention of a jargon containing such words as haptophore, ergophore, complementophilogen, amboceptors, agglutinins, precipitins, bacteriolysins, opsonins, syntoxoid, and so on, which again bred other equally futile words of the same kind, was but to stultify real explanation, and to cloud perception of the actual facts. When all this was hung upon the giant molecule, a mere guess of Verworn's, the structure naturally enough came to the ground. But, though the whole theory of side-chains, except so far as they are mere chemical phenomena, has been discredited, there are yet able bacteriologists who continue to teach it as if Ehrlich were accepted gospel. On my inquiring why they did so, when they knew that the theory was no longer held by any physiologist, two or three well-known men replied that it was useful to students as a framework on which to hang facts. To this it might be objected that, though a hat-rack is useful to hang hats on, yet it is not the place to hang hundredweights, and that when the hat-rack has the additional disadvantage of being an imaginary one, there will be more than common difficulties in the way of useful arrangement. A compound may, perhaps, be cleared of cobras by a mongoose; but no competent zoologist will employ the "imaginary mongoose" of fable at the task. To teach what is not true merely as mnemonics is to ignore the truth that any fact falsely classified is not only a danger in practice but leads to false views with regard to other sciences, to say nothing of the distrust of the teacher evoked in the intelligent student who does not swallow what is offered to him without producing an "anti-body" to the professional "antigen." For such a student may have acquired some smattering of colloids, and have actually begun to see, as Moore wrote, that the pioneer work of the French "had been burdened with the intolerable weight of a useless philosophy of jangling terms for a type of reaction well known in colloidal chemistry." It will probably be found that a dictionary containing the common terms of chemistry and biochemistry is fully sufficient for the reactions even of complicated colloids. An obscure reaction is not explained by attributing it to an imaginary substance with the very qualities which are the subject of investigation. To invent one is to fall into the error which Molière satirized. To say all this is not to belittle the magnificent practical results achieved by bacteriologists.

That the results of the search into the actual nature of immunity have apparently been so barren and so confusing, is, however, not due to want of suggestions which might really work for simplicity. Yet, so far as I am aware, Moore and Whitley's paper on a simple theory of immune reactions has by no means had the attention it deserved. Roughly speaking, their note put forward the view that immune bodies were to be classed with catalysts; the substrate being the cell or bacterium to be dissolved, or the toxin to be rendered inert, the "complement" various bodies with which the toxin became chemically united, and the immune body, or "antibody," the catalyst which insured such chemical combination or dissolution, and rendered the pathogenic cells or toxins harmless. Such a process is obviously exactly similar to those which occur with enzymes acting on any given substrate in the presence of a combining body: so tyrosinase breaks up tyrosin, and causes it to combine with the oxygen yielded by peroxide bodies. A similar process occurs with the ordinary hydrolytic enzymes, as when fats, carbohydrates, and proteins combine with water in the presence of their specific enzymes, such as steapsin, amylopsin, and trypsin. It may, however, be further suggested that all these actions are really immunizing actions, and that, instead of immune bodies being classed among enzymes, the latter should be classed generally among immune bodies, and both among catalysts, the difference between the first two classes being that enzymes dealing with food are a gradual result of evolution, and that what are usually called immune bodies are special *ad hoc* reaction complexes of a similar order depending on the general powers of reaction in the body tissues.

It is obvious that foods when not broken up are either poisons or something which cannot be used, and must be excreted. Thus proteins injected into the blood-stream are hæmolytic. To be endured or used they must be broken up into animo-acids. What particular quality it is in them which makes them "antigens" is obscure; but it will certainly be found eventually that it is due in all cases to their "poisonous" (*i.e.* disturbing) action, since they are wholly out of their evolutionary place. It is said that there is no absolute relation between toxicity and defensive reaction; but this is only to state the obvious fact that the organism is not armed at all points, and may be destroyed before it can react, or that it is already supplied with general immune catalysts which deal easily with some invasions and fail with others. It has only been through an immense period of evolution that the proteins and other food elements have come to stimulate the production of specific enzymes or catalysts, and it is within every clinician's knowledge that in certain conditions of health these necessary reactions do not occur. When that is so nutrition fails, and food becomes a poison. If this is correct, and it cannot be doubted, nutrition must be regarded as an actual process of real immunization, in which secondary and simpler products are used by the organism as food. Although I came to these conclusions before I was aware of their work, it appears that Abderhalden and Weinland hold views of this kind. Weinland states that the subcutaneous injection of cane-sugar produced or elicited invertin. Abderhalden's work on the production of gestational immunity has exceptional value in that it throws some light on the inhibition of the invasive action of the chorionic villi on the uterine wall, and thereby on cancer also, as I have suggested elsewhere. In the earlier stages of evolution, in all cell life now, and that of the intestinal absorption cells, the ingestion of foods is due to purely physical causes, *i.e.* causes which are not sufficiently obscure to be labelled "psychic" when they should be regarded as conditioned reflexes. Like and dislike, choice and rejection, had, and have (as regards cells), no long path to depend on. Such reactions are not even simple reflexes. They depend entirely on surface tension, on the nature of the cell envelope and the body with which it comes in contact. It may be assumed that primitive cells which certainly possessed low and scanty reaction powers took in all things which their physical nature did not reject. Some were innocuous, and some were at once ejected. Some were harmful, and destroyed the organism in which they found themselves. Others were harmful as they stood, but by producing a reaction body they were broken up and rendered harmless. Others, again, were not only rendered harmless but actually useful: *i.e.* they became foods because they provoked a definite catalyst which hydrolyzed them. Like and dislike of foods in the highly developed organism are thus conditioned protective reflexes which defend the body from all but foods selected through evolution. Nutrition thus clearly falls under the head of immunity. J. B. Farmer remarks that they are evidently closely related. The sole real difference is that, in what is now called immunization, the substrate is probably not employed usefully, though it remains possible that in some cases destroyed pathogenic bacteria may actually be used by the body they attack. In any case it is a possibility of evolution for bacteria to become gradually a factor of further growth. Such phenomena may have occurred already, as it is at least possible that the colon is partly a function of the bacteria that inhabit it, and that some dead bacteria may be converted into food after their parasitic free existence.

Nutrition being then a case of immunity, we must infer that food itself originally produces *ad hoc* catalysts, of an order similar to those produced by toxins. Enzymes very rarely exist without the presence of their substrate. The organism has a specialized method of producing them on definite stimulation. Thus trypsin only appears on trypsinogen coming into contact with enterokinaze, though it would be better to say that when certain colloidal reactions of pancreatic origin take place in the presence of the latter trypsin is formed. Lactase cannot be found in a meat-fed dog till some days after it is given milk. Before such a reaction occurs the whole of the food products in the milk are not utilized. The complementary or combining bodies in oxidation and hydrolytic processes are O and H_2O, and in all enzyme action there is a natural "complement." In the production of special immunities due to infection it is difficult to discover the combining body, although in most acute infections it may be lipoid in nature, as is certainly suggested by the rapid consumption of fats in fevers. Yet in diseases such as typhoid, in which emaciation is not rapid, this does not appear to be the case. The combining body in this case may not be a lipoid but an albumenoid, and perhaps the special one found in Peyer's Patches, where ulceration takes place, is most exposed to be used as complement. In many cases of sudden and extreme weakness with a high febrile reaction, it may be suggested that the infection is proteolytic. It thus appears as if future treatment may not only include the provocation of the special catalyst, but the early supply of a definite complement by injection or in the food. In most infections it is probable that what can be most easily spared goes first. It is possible that the combining body is always what the bacteria would naturally feed on, which may serve to explain latent periods and the slow onset of fever in many cases.

What, then, is the action of the catalyst? It must be that it builds up in the bacterium a stable compound, and so destroys the labile organism that takes in complement as food, or that it neutralizes the toxins as they are produced. Or it may so completely alter the combining body that the bacterium starves. As regards "free toxins," the catalyst fulfils an obvious function by its usual machinery. I suggest that the greatest function of the many attributed to the phagocytes is not their ingestive powers, but their capacity of being used as "complement" or a combining body. If sufficient of their lipoids or proteins, say, is used up, they die and become pus : if not, they survive and destroy the bacteria which were attracted to them, and probably redigest the lipoid complement taken up. In any case, the entrance of the bacterium or toxin into the so-called phagocyte is probably pathological. They may recover or perish. If they die it is because they are used up in yielding parts of themselves to the hostile cell. Every cell envelope has lipoid substances in it; but the phagocytes that cannot yield any more from the outside yield it from within and are destroyed. Even if that is not the cause of their death, they may die from toxins entering into chemical or molecular union with the lipoids of the membrane, which is rendered functionless. If these suggestions have any foundation, the leucocytes generally have another rôle in defence than that commonly stated. And we may infer that the substrate "seeks" them and their lipoids under the influence of the special catalyst, rather than that they "seek" the substrate. If this is so, opsonins do not exist, and the phenomena they are supposed to explain represent the fact that the easiest reached "complement" is found by the tropisms of the bacteria and the polynuclears. The leucocytes are the cheapest sacrifice the body can make.

There seems plenty of evidence that enzymes and all catalysts appear only when the organism is stimulated by the particular substrate with which they deal. There are, however, according to Bayliss, cases in which their particular substrate is wanting. For instance, it seems that lactase is found in almonds, as adrenalin is found in the skin of the toad. We say, then, that it is an accidental product of metabolism, though in one sense all such are accidental. They may, however, have been provoked by early embryonic substrates, and possibly subserve some function. Onslow showed the skins of some coloured rabbits contained a peroxidase, the cause of the colour. The "adaptation" of the organism to any substrate part of its environment must have been accidental to begin with. Some cells had the power of response, and others lacked it. This was so in the early stages of evolution, and the same remains true now in every case of recovery or death when an infection occurs. Protective, continued, and successful reaction *is* adaptation. There are striking analogies between such reactions, and those provoked by drugs such as the metals. What for instance is the action of arsenic? It combines with the "complement" it finds, and thus kills the epithelial cells which, according to Filehne, are then digested. But killing a cell is combining with it or part of it. Arsenic in lethal quantities is such a protoplasm poison that there can be no swift reaction process resulting in a catalyst which builds it up into a harmless stable compound. In small continuous doses it appears to produce fats, *i.e.* possibly a superabundance of complement. Complement is thus a common bodily product, not anything specially manufactured by specific reactions, and therefore immunization must in many cases mean a stimulation of the cells which produce complement naturally. Immunity to arsenic, then, is most likely due in great part to an increased production of lipoids. But tolerance *is* immunity. Immunization is thus a process as normal as digestion, save that all the products are finally extruded as useless. The processes leading to cure are of the same order. To give a homely illustration we may say that if there is disturbance at a meeting the disturbers are the substrate: the chairman who orders them to be thrown out the provoked catalyst, and those who act on his advice the complement. We may even note that the "complement" not unseldom goes to hospital in combination with the "toxin." In such cases immunization is the operation of a body of stewards capable of immediate and skilled combination. It is worth notice, since it has frequently been suggested that all living action is based on the same principles, that the stewards are free amœboid wandering bodies; while the audience of cells of the temporary organic body we call a "meeting" stay where they are, unless there is a violent destructive reaction. In what we commonly call an organism the white cells, being capable of rapid reproduction, and not stationary portions of basal functioning organs, can be destroyed and replaced. Part of their normal function is to die, as it is of soldiers in war. Adequate military preparation is expectant immunity in a nation, and an organized police force means the same in a society. Again, it can be repeated that much light may be thrown on many obscure physiological problems by observation of the simple social processes taking place before our eyes. On these lines we get wholly away from Ehrlich, and perceive that, if enzymes, etc., are immune bodies, immune bodies in their turn may be classed with the factors of digestion among catalysts generally.

What do we mean when we speak of the bactericidal qualities of the blood? Undoubtedly the blood-plasm, when healthy, destroys or incapacitates invaders. But what is the mechanism by which it does so, and among what phenomena are we to class such mechanistic reactions? I am unable to conceive that they can be any other type than that which is characteristic of life generally.

There must be activating catalysts. If that is so blood may not be truly bactericidal until it is invaded, and only then if it is healthy, *i.e.* if it possesses normal powers of reaction. Such views account for the high mortality among the very healthy in some disorders, while the sickly person who is, perhaps, half-poisoned by an excess of many provoked catalysts is prepared, at least partially, for any kind of invasion. The organism, too, has to deal with, destroy, or utilize its own products. Each organ must deal with its own excreta, with the excreta of its neighbour colonies, and for these purposes reacts on their stimulation. Its reactions with invaders must employ like machinery. Life itself depends on immunization which is active warfare.

Immunity, however, does not always seem to be merely a matter of the increase of the blood's bactericidal qualities, to whatever catalysts that may be due, but to local conditions. How else can relapses be explained? For instance, there are the relapses of typhoid fever. When the fastigium has been reached and passed, and when on any theory a defence should have been acquired, the temperature again rises, and there is another attack. In the same way a catarrh of the lungs may disappear after a due reaction period, and another patch will occur. Such facts, though they do not negative the ordinary views of immunity suggested above, at least show that prolonged febrile reactions do not always produce complete temporary immunity: the most probable explanation being that the local lesions in these cases are external, as the bowel, say, in typhoid is properly external, and are with difficulty exposed to the immunizing agents, owing to local swellings and stasis, or to the incomplete response of the weakened organism in the production of the combining body. The catalyst may in that case be present in abundance; but it has nothing to work with, and the substrate flourishes on tissues which would not be attacked if the normal complement were present. In some cases catalytic actions may themselves prove harmful. It is perfectly possible that the obscure and dangerous phenomena of anaphylaxis are due to a sudden action of a reversible catalyst breaking down what was before built up, and again setting toxins free. That the immunizing reactions are permanently reversible if a disassociation factor such as acid (Morgenroth and Ascher) or alkali (Sachs) is introduced, now seems certain. These workers are quoted by Browning (*Brit. Med. Journ.*, Dec. 6, 1915).

Such conclusions certainly reinforce the suggestion that in all infections an effort should be made to determine with exactitude the natural combining body used in the body's defence, and that this should be supplied in abundance by feeding or injection. On the views expressed the lipoids or other combining bodies found normally in the organism would then be spared, since it is only reasonable that a free lipoid, etc., would be dealt with rather than a fixed lipoid, say in the cell envelope. While vaccines may provoke the catalyst, they cannot always provoke the "complement," although when we consider some drugs (and tolerance of drugs, as has been said, no doubt comes under the head of immunity) they may do so indirectly. Though arsenic when used as a drug is often useful, its value may not always depend on its bactericidal qualities, but on its encouragement of lipoid manufacture.

Thus finally we see that nutrition itself is but a case of immunity, and, instead of immunity being infinitely complex, on a general view it is no more than an example of the fact that living protoplasm develops machinery to deal with the assaults it undergoes; in some cases changing and making use of what it ingests, in others altering bodies which cannot be used, but may be made harmless.

The points of the likeness between a catalyst and an engine or tool are many. It is something made, a reaction to needs. It works and remains the same. It only wears out gradually. It makes a long process short, a difficult one easy. It can build up and break down. Such qualities can be seen in a sociological example if we take two living communities, or societies combining for a common purpose, for then combinations will result which have the most remarkable likeness to such catalysts, and exhibit biological phenomena which throw a light on the reason that enzyme bodies have reversible reactions. We may, for instance, suppose that a botanical and a zoological society find it necessary for purposes of existence to take a common house. It is obvious that we have here a symbiotic but not necessarily a symphilic community, and it is equally obvious that both original purposes must be served, or the whole structure fails. To carry on work some of the individuals must represent the others, and this common secretarial office remains as a formed reaction body or engine for getting the work done. When one society is most active at any given time its action may be regarded as synthetic so far as it is concerned, and the process implies a temporary displacement or breaking down of the others. When this is over the reverse action takes place. But all the time the formed reaction or joint secretarial body remains. An enzyme can thus be compared with the common alternative executive of at least a double body which has different internal but common external ends. However extravagant such a homely illustration may appear it probably represents actual biochemical facts; for it shows how two molecules living in hostile symbiosis must affect each other and produce reaction bodies which are highly complex, and represent in turn the energies of each molecule. Such a view affords a reasonable ground of explanation for the reversible reactions of catalysts themselves. Without such reactions it is not easy to follow the possible ways of growth.

It will not seem absurd to some if it is said that these likenesses are to be found at the bottom of all growth, whether of atoms or nations, and that they are part of a universal law which may be expressed thus: All life and growth is fundamentally the forced result of a symbiosis of differing bodies in which hostile energies become the common, mutual, and reciprocal internal stimuli of the conjoint individual. This includes all living things from the two molecules (or more), each with a catalyst, which probably make up the simplest form of life.

To summarize the views expressed it may be said that—(1) To understand immunity it must include all processes of nutrition. (2) All unsplit ingested bodies are "poisonous" or rejected as neutral and useless. (3) Enzymes are the catalysts which build up for storage or break down for use. (4) Immune bodies in infections are provoked catalysts dealing more or less well with the substrate bacterium, or toxin. (5) Poisons, metallic or alkaloid, etc., when tolerance is established, have provoked a catalyst to deal with them. (6) "Complement" is not a fixed quantity, but the special or general combining body used by the catalyst and the substrate. (7) Many of the difficulties experienced by bacteriologists in reaching satisfactory conclusions on immunity are due to their neglect of colloidal chemistry.

It certainly appears that the terms employed in general physiology should be sufficient for bacteriology, and observers of fresh phenomena ought to be chary of coining new words. Their hasty multiplication usually implies some additional hypothesis. It is characteristic of a false explanation to require an increasing number of sub-hypotheses while a real one abolishes a multitude of superfluous terms, and, displaying a phenomenon as the function of known variables, by such a disclosure becomes essentially a simplification.

REFERENCES.

ABDERHALDEN, EMIL.—" Biological Reactions in Pregnancy," *Brit. Med. Journ.*, London, Jan. 24, 1914, p. 207.
BAYLISS, M. W.—" Principles of General Physiology," 1915.
BROWNING.—*Brit. Med. Journ.*, London, June 2, 1915.
FARMER, J. B.—" Plant Life," 1913.
MOORE, B., and WHITLEY.—*Biochem. Journ.*, 1907, London, vol. iv. p. 165.
MOORE, B.—*Brit. Med. Journ.*, London, Dec. 22, 1917.
MORGENROTH and ASHER.—*v.* Browning, *ibid.*, Feb. 6, 1915.
ONSLOW, H.—*Proc. Royal Soc.*, 1915, Book xxxix. p. 36.
ROBERTS, MORLEY.—*Brit. Med. Journ.*, London, Dec. 8, 1917.
SACHS.—*v.* Browning, *ibid.*, Feb. 6, 1915.
VERWORN, MAX.—" Die Biogenhypoithese," Jena.
WEINLAND.—*Brit. Med. Journ.*, London, Jan. 24, 1914, p. 207.

CHAPTER VI

The Cannibal in Evolution

We speak very commonly of the romance of science, and in its every branch, however recondite and apparently remote from human interest, there is that sense of adventure which makes its votaries thrill with expectation. The search for a principle which may throw light upon almost palpable obscurity is not unlike the work of the explorer who climbs a peak in order to discover a way through the unknown. Not only the traveller stands "silent upon a peak in Darien." But to discover a new general law, or even to extend one, is far more than such a distant prospect. It is to camp in the wilderness and gather strength for the morrow's task. In the world of geography what is known is known, and cannot be forgotten. The time must come when the explorer's work will be done. But science is limitless, and so is human history, for there is not only the future which must become at last a tale that is told, but also the illimitable field of the past still as little unmapped as the fabled Africa of the mediæval cartographer. It is, indeed, but a dim and faded palimpsest, or some inscription in an unknown tongue, of which we know but a few words, that may prove keys by which it may be deciphered. What of the march of power and intellect in man it may reveal we can only guess, though we may surmise from human experiences and our own nature that it must contain terrible passages, which some, perhaps, would fear to look upon.

In the study of anthropology there is, perhaps, the most legitimate field for the constructive imagination. So much may be seen in the realm of history, the brief portion of the story of man with which we are partially acquainted. A history without imagination is but a false and dusty record, a sketch in black and white of what was once a glowing fresco. The shelves of libraries are full of such dead documents, and only occasionally does the reader light upon a work, or even a passage in a work, to which the realizing imagination has given a sense of motion and life. Such a passage is to be found in Professor Murray's *Rise of the Greek Epic*, when he describes vividly, and with convincing power, the probably course and conduct of an early migration in the Ægean Sea. It is a matter for regret that even anthropology, with all its immense implications, should exhibit much of that restricting tendency of scientific men to confine themselves to special rather than general fields. Yet just as its study throws light upon the conduct and behaviour of living races, so many special studies, that, perhaps, of Pelasgian myth or history, may help to solve the mysteries of the unhistoric past. Even if it does not do so directly, it will show specialists that to ignore the imagination is to deprive themselves of the most powerful weapon in the whole armoury of research. If in the course of some daring and slenderly based speculations their rash author makes one possible suggestion, he is as much justified as the poet who writes but one memorable line. In such a fluctuant, inchoate branch of learning, it cannot be said that knowledge has yet reached the period when theory is held to be dangerous, which is, I must suppose, the time when the supporters of some particular view wish to see every one's energy consumed in the search for facts to uphold it. But, to alter the phrase of the Master of Trinity, none is infallible, even the oldest, who not unnaturally are apt to regard their settled opinions with the greatest respect. A new hypothesis, or a new application of an ancient one, is, however, in its way as much a fact as bones in a tumulus, and may perhaps do work as important as the Piltdown skull.

Among the curiosities of the human intellect is its great reluctance to acknowledge anything now regarded with moral reprobation as once normal to mankind. The tendency of all races to place the Golden Age in the past, which is the result of a dread of change, since any alteration may bring disaster, and must cause temporary disorder, acts thus as a forgotten complex even in science. In spite of some authorities having recognized the probability that all races have passed through a stage of cannibalism, many others of equal or greater prestige appear to regard the notion with horror.

It is objectionable to their moral feelings, and, although it is the first duty of the scientific thinker to clear his mind of prejudice, some are obviously unable to do so. Accordingly one very great authority practically asserts, not without feeling, that horror of incest is a primary feeling in man, an abstract notion thus preceding experience—a totally impossible position to occupy in any "science" but theology. As it happens I myself encountered this moral feeling with regard to cannibalism, when in an essay, published in an obscure journal more than twenty-five years ago, I attributed to it very great and important evolutionary results. It is true that no notice of an immature paper was taken by any authority, a fact I by no means resented, although hardly then fully aware that any variation in thought, like a variation in the physical conformation of an individual, is more likely to be swamped than perpetuated. Since then, in discussing the view suggested, I have found the moral complex showing itself, at times with almost theological ardour, even in the instructed. They seemed willing to agree with M'Cullough and others that a stage of cannibalism might have been universal, or nearly universal; but to view it as a powerful factor of progress and human advance was something not easy to masticate, much less to swallow. Yet some reason may possibly be shown for believing that cannibalism, combined with war for a special purpose, can help us to account for many problems yet unsolved. Even a very simple suggestion sometimes acts as a catalyst, and hastens mental reactions. It may often help to neutralize moral prejudice, and increase in some measure the number of those who have learnt to adopt Spinoza's attitude towards humanity. Man's acts, his very perturbations of "spirit," are to be studied as we study thunderstorms.

If such views are not refused a hearing it may be suggested that they will throw some light upon the development of man as an intellectual animal. And if it is assumed, as certainly seems likely, that Keith is right in tracing backward to Pleistocene times the modern type of skull, it should help to fill up the gap in development between that type and such as Piltdown man. For if the modern skull goes back so far, and the Piltdown type is so late, the very stability of the modern type suggests that there must have been a period, long in years or centuries, but short by the geologic clock, in which man became immensely plastic and changed with relative rapidity. The evidence seems quite ample which tends to prove great stability of type after the middle or late Pleistocene era, and such stability shows to all who believe in environmental influence, or in natural selection, that since then there has been no great fundamental change of moulding factors. Some, indeed, imagine that what is called civilization has been such a factor. This is practically assumed by most who hold that " modern " man is historically modern. But, as any change seems great to individuals who are disturbed, it is natural for most to come rapidly to the conclusion that great past political and social changes may have had, and perhaps must have had, an evolutionary effect even if comparatively recent. Yet as most changes are now but new orientations in thought, which do not lead to the destruction of established physical types, and as such factors of selection as ill-health, defective mentation, and so forth, have continually operated from the dawn of life, no vital factors can be discovered working at the present time which suggest the new and rapid evolution of a cranial type. If such factors worked, one or more of them must have disappeared. Since the ancient complex of imagination, fear and regret has ceased to picture the happy golden ages of the past as a restful paradise compared with the dim uncertain paths of the future, even the most conservative only employ their imagination in constructing ideal scenes in a blissful state of ordered feudalism, and for very many it has become a habit to picture themselves as the apex and acme of possible mankind. So indeed it was in the past, for even if the poets of Greece and Rome looked backward to the *Saturnia regna*, as the tribes of Central Australia do to Alcheringa, they would yet maintain that they had reached a summit of civilization from which only a descent was possible. Mankind thus tends naturally to believe that great attainment, even if fabulous peace has passed away, is their own work, and the work of their immediate ancestors. It follows that there is a natural prejudice against admitting that, in all human powers and attributes of brain, man's remote ancestors were his equals, even though they lacked his present knowledge. To get rid of such predispositions is part of the task of science, if it would solve the problem as to the factors which were at work at the time of man's greatest cranial plasticity.

While it is not difficult to believe that Mousterian man possessed an average cerebral capacity more than equal to that found now, some find it hard to credit such brains with imagination and powers of logical thought. Yet Fraser has shown that many who are called the lowest savages reason with perfect logic, even if they argue from unexamined and illicit premisses. It ill befits the average man of the present day to cast a stone at them, since the subjection of his own major premisses to critical examination invariably causes him much uneasiness. Even the greatest are at times subject to the same weakness. Ancient man was always reasoning and, since pure logic has nothing to do with the truth or falsehood of propositions, but only with their agreement, there is no reason to suppose that a school logic of merit might not have been composed from a study of the ratiocinative processes of the earliest modern type of brain known to us. We may, indeed, analyse further, and in so doing discover that logic is to be found in even lower human types, or in the animals themselves. A cat who smells a mouse, and takes means to catch it, is using direct inference. Moreover, in considering the evolution of the imagination and intellect we are apt, according to Keith's views, to think that when we make great discoveries they must be relatively greater than those discovered in the past. Yet it may be doubted if Napier of Merchistoun made a greater discovery than the unknown genius who first counted on his fingers. It is at least certain that, though logarithms lessen labour, they have not lessened it to the millionth extent that finger reckoning, and all that has flowed from it, including logarithms themselves, have since achieved. To learn how to make fire was a greater discovery than any made by Watt, while the inventors of the wheel or the wedge must have been men of the very highest capacity. The same may be said of the arts, for the discovery that an outline represented in some magical way a real animal or a person, whether it was found out by some savage boy outlining a shadow, cast by the camp-fire, on a neighbouring rock, or by some primeval master, was an effort of much more amazing originality than a masterpiece by Rembrandt or Rubens. We cannot, perhaps unjustly, attribute most, or even many, of such inventions to a Piltdown brain. The question is then how it came about that relatives, close or far removed, of *Homo Eoanthropus* gave rise, within a comparatively short period of time, to the later and still prevalent type, capable of the highest intellectual efforts. A solution of the problem may, perhaps, be found in cannibalism as the chief factor, if it first gave rise to organized war and the development of weapons, such as made the best period of Chellean art a time of masterpieces in flint.

It is certainly justifiable to assume that some such factor is needed for explanation. If the missing link is not so much a type of man as a missing page of human history, of which the previous and following parts show immense changes, we are equally within our rights in filling up the lacuna by the use of an adequate hypothesis as we should be in supplying to a tragic play a missing scene which, to render later acts possible, must have contained a murder. In such a mutilated script there is a strict parallel to what was probably the most tragic part of human history. The play would perpetually suggest the action of the missing portion, and so, in later and modern history, and in the instincts of man, we have hints, and more than hints, that obscene tribal survivals represent historic universal truth, even if nothing is said here of cave remains, skulls, or bones, which are the island peaks of the submerged continent of anthropology.

If such deep seas cover that lost land they may yet be sounded, and, as it were, dredged, so that in the end, by actual evidence and logical inference combined, the unknown may be mapped out. If we judge from what remains in those savage customs which offer the best means of deduction, we get lines pointing in definite directions. If more than one line indicates the same solution, the inferential value of both is much increased. Such a method is similar to that by which bee-hunters seek the tree-hive where they look for honey. By the observation of the flight of the insects on their homeward path they obtain lines of triangulation which are a sure guide. As regards the early history of man one such line may be, perhaps, found in Atkinson's *Primal Law* — the repository of views too much neglected. From a study of "avoidance" in forms well known to him, its author at least deduced something of the nature and origin of the primal family, its laws and customs. He pictures the savage ape-like ancestor of man as the father and husband of all his female children, as well as of the stolen women who bore them. An immense and overpowering sex jealousy led to the extrusion of the male offspring when they reached the age of puberty. Such sons broke into the sanctity of the family circle dominated by some other ancestor of man, and set up for themselves. Incest at that time was not intercourse between father and daughter, but between brother and sister, son and aunt, or mother, and the penalty assigned and exacted for ages was death. According to Atkinson, and here I by no means follow him, such a system was probably broken down when the patriarch grew old by the exceptional influence of some " wife " who retained great maternal love for her latest grown-up male child. It seems that other more widely operating and less abstract motives can be shown, which must have exerted their influence on the husband and father of the camp and all its members.

It is now some fifteen years ago since I deduced from certain social phenomena, which I propose to indicate, a particular theory of the family and the two-class tribe. This I submitted to the late Sir Laurence Gomme and Dr. Haddon. After some consideration they referred me to Atkinson's paper, reprinted in Lang's *Social Origins*, as they were of opinion that my views had been largely anticipated by him. I discovered this to be a fact, and for a short time experienced those feelings of indignation natural in one who believes himself a pioneer, and finds a camp pitched on what he thought an unknown territory. Such feelings did not last long.

When a hypothesis is immediately verifiable, and discovery is anticipated by others, there is little or no compensation. If Adams had been forestalled by Leverrier in the discovery of Uranus by long months or years, and the planet had actually been found for the more fortunate mathematician, the sense of disappointment must have been acute. But in my own case no such verification was at hand, and I was quite aware from the attitude of those who advised me, and my own knowledge of theory, that Atkinson's views were thought of little value. Naturally they did not seem so to me, for while he had deduced his theory from the facts of the social phenomenon, known as "avoidance," of which the last remnants are to be found tabulated in the ecclesiastical *Table of Affinities*, I had come to the same conclusion by a deduction from opposed and antagonistic phenomena, the existence of which his views helped to explain. For if he drew his premisses from savage life and "avoidance," I drew my own from certain facts observed by myself, and even now observable by all, in modern society whether in England or elsewhere, facts not of avoidance, but of a peculiar form of jealousy, which seemed to me an obvious survival of ancient instinct.

It is a truth, known to almost all wives and to women generally, whether they have observed it in their own husbands and fathers, or in those of others, that something more than an over-exigent desire to ensure their female children being comfortably and suitably settled, prevents many men from welcoming suitors for their daughters. And I may say here that the deductions I draw from this are probably seldom known to the fathers themselves. To act instinctively without the knowledge of the why or wherefore is common enough, and the origin of many surprising facts can be explained by certain developments in modern psychological theory and practice. Not a few men object to the presence of other men in their house, especially when they are not present, even when all thoughts of purely marital jealousy are wanting. They even object to the "party," that feminine function which tends to lead to love affairs. But the main fact is that the resistance such show to the marriage of their daughters cannot be explained on acknowledged social principles. The young people may exhibit every sign of true affection, the suitor may be of good character, even of high social standing, and yet the father will raise every imaginable objection, and put every conceivable obstacle in the way of the desired marriage. In many cases I have known the young men forbidden the house on the mere ground, the last the father had, that he did not like the man, whom he slandered in the bosom of his family without being aware he was precipitating flight and an elopement, which is the modern form of marriage by capture. There are cases, well known in later literary history, in which, after furious struggles, this result has occurred, and in biographies we see the parent's resistance put down to anything but its real cause. That is said to have been his great affection for his daughter, her necessity in the house, her father's need of companionship, or the needs of her mother; but never in any instance to deep-seated sex jealousy of instinctive origin. Without relating in any detail my own observations and experiences in this matter, I may say that on making inquiry of many men with daughters, quite a number of them owned that they had seen in others what I had seen, and a few, who had been accustomed to the analysis of motive in various branches of medicine, actually admitted that they had felt the very emotions of sex jealousy I have indicated as yet existing in modern society. Moreover, on consulting a lady, herself no mean anthropologist, she declared the phenomenon, whether understood or not, was known to all women by experience or report, and that it was not infrequently hinted at in private feminine conversation. The more observations I have made the more I have been convinced that the facts are as stated, and it was from the hypothesis that even now large numbers of men, without any desire of possession apparent to themselves, are sexually jealous as to their female children, that I deduced the same conclusion that Atkinson had reached by the opposed but complementary hypothesis of avoidance. It may here be remarked that the common sex coldness between members of the same family is thus not due to being brought up together, as commonly supposed, but that it is the last result of the system of avoidance become instinctive in boys and girls by long ages of inheritance, in which the penalty of infraction of parental law was death. Such a conclusion, it may be remarked, is against the view that the young men sought their wives at any time in the camp from which they had been extruded.

It should, however, be made quite clear that these ancient surviving instincts are not so vocal or so clamourous as to speak clearly in those who now retain them. The most jealous parent of the kind may not have the slightest notion of the reasons moving him. As far as each successive suitor is concerned, it is to him a case of "Dr. Fell," and there is an end of it. But many are distinctly conscious of the facts, though such consciousness is in many cases sedulously hidden, even if acknowledged, as I have reason to believe, in the confessional or the physician's consulting room. I know that such an analysis, however supported, is as little likely to meet with approval as the suggestion that cannibalism itself is responsible for much of our mental make-up. The reception of the idea, even by many of those who might be supposed to view all things in the "dry light" of the Freudian psycho-analysis, has been almost one of tumultuous opposition, although the light that Freud casts, both on normal and abnormal cerebration, has been of the utmost value. It may, I think, hold a lamp even in anthropology, and it is possible that certain conclusions drawn by its aid in the matter now discussed may strengthen the growing belief in it as a weapon of discovery. According to the views expressed in other places, it is far more than probable that what proves of value in individual psychology will aid to unravel the tangled web of racial subconsciousness in which the instincts have their root. Few are now totally ignorant of Freud's work in the analysis of the subconscious mind. However they may look upon it, or upon some of the extravagances of its more indiscreet advocates, not many can be found to deny that the hypothesis of hidden complexes, by which is meant a series of cerebral reflex arcs still in a state of subconscious tone and capable of producing peculiar effects, has exerted an immense influence on the theory of conscious mentation, or mental action. An early impression, although forgotten, or dissociated from the general web of memory, since a memory can only be the repeated passage of impulses over many definite synapses, may condition for better or worse the whole life of the individual to whom the incident has happened. It has not, however, been general to attribute good rather than evil, health rather than disease, to such unconscious memories. But kindly, or thoughtful, or unselfish acts may leave their mark in the same way, and a sound early education is, perhaps, no more than the excitation of such useful complexes. More is definitely known, however, of the evil effects of painful forgotten incidents which often yield to psycho-analysis. By a skilful use of morbid symptoms, shown in myriad forms, the operator may link up the past with the present, and demonstrate to the patient the trifling origin of his ills. To do so seems to be the drainage of what we may call figuratively a mental abscess. I am not aware that any one has suggested that the human brain or mind, the depository of the racial subconsciousness of man, must show in its very constitution similar phenomena. There must be human deep-seated hidden complexes determining thought and action, and showing, if we could read them aright, through what avenues our ancestors have passed. When some hidden complex, which might have worked morbid results, has been sublimated, as the psycho-analysts call the process, the hidden repressed energy makes a healthy path for itself in the brain, and lifts up such things as repressed sex feelings into devotion, altruism, and even self-sacrifice. Such must have been part of the method by which the racial type of brain has been developed, and so certain do I regard this that I think it might have been possible to deduce from racial history, folk-lore, and even written history, the very theory of psycho-analysis itself. Myth and legend often enough are sublimations, but in the deep melancholy, or unmitigated brutality of some races, may be detected ancient influences not so fortunate in their issue. Reversion to ancient type in the masses of a race, a subject not remote from us, is rendered easier to understand. For in their brains lie quiescent the memory and actual cerebral machinery which once more may be set working by some great stimulus. War could not be the passionate relief in action that it is to many, if its memories were not graven deep even in the peaceful. So now, perhaps, in the night-horrors of children or their elders, there may be some dim relic of ancestral fear, which many childish tales partially awaken. Such a view is in tone with the general purpose of this paper. The ogre or giant who eats children is thus a reality to them, for their ancestors dwelt for unnumbered centuries among such fearful possibilities. The very character of women, with their powerful, but half-hidden, insistence on success, could thus perhaps be traced, without the idea seeming over-fantastic, to the times of ancient famine when their children had, perhaps, to fear most their natural protector, even, it may be, the mother herself. It is, indeed, far from unlikely that it was the women who urged on their father and husband to the capture and slaughter of his enemies, and his own fear, as I hope to show, may be held up as the one great cause of his sullen co-operation with his sons in such expeditions, enforced and enjoined upon him, though it may have been, by some favourite woman who was apprehensive of disaster to her young offspring.

To return from these relevant deductions to the parental jealousy complex still showing itself in modern times, it seems that many such ancient social states must have perpetuated themselves in mental complexes which still influence human action. They are but examples of the ceaseless working of energy ever and ever in weaker but more delicate structures. So, in the growth of the nervous system, embryonic cells, capable of development into muscle cells which use great energy, were, if we may use the metaphor in physiology, sublimated into nerve cells, which consume so little that it cannot be measured by any means yet known to us. But, just as we know that neurons arose from ruder and more energy-consuming structures, we can infer that, though the finer and more delicate instincts of modern man were developed from rude and brutal ones, they still retain marks of their origin. Nor need we be surprised to find even now in such a lowly organism as society, which lies far down the developmental scale, obvious or gross indications of their origin. With this support, and the coincidence of Atkinson's views with those otherwise deduced, some progress may be made in the consideration of the conditions and factors which changed the ancient typical family of the father, his wives and daughters, and the children of both, into the tribe. In such an investigation it will not be necessary to go into later developments, such as matriarchy, which were probably due to special, perhaps local, causes.

It is the common accepted opinion that tribes grew directly from the family which co-operated as an ever-enlarging unit, and afterwards subdivided. Such a view as much ignores the political phenomena of history, even of to-day, as it does the many sidelights which ancient custom throws upon the processes in question. When any opinion is based upon little evidence it often turns out that the effect is mistaken for the cause, or the cause for the effect. It may, perhaps, be shown that something like this has occurred in the conclusions based upon the classificatory system of relationship and inter-tribal customs, known chiefly to us by what is seen among the Australian aborigines. In order to show as clearly as possible what the orthodox view seems to be, I may quote Frazer (*Folklore in the Old Testament*). After speaking of the exogamous classes of a tribe as always two, four, or eight, but never an odd number, he says: "This suggests, what all the evidence tends to confirm, that these various groups have been produced by the deliberate and repeated bisection of a community, first into two, then into four, and finally into eight exogamous and intermarrying groups or classes, for no one, so far as I know, has yet ventured to maintain that society is subject to a physical law, in virtue of which communities, like crystals, tend automatically and unconsciously to integrate or disintegrate, along rigid mathematical lines, into exactly symmetrical units. . . . The evidence points to the conclusion that the dual organization or division of a community into two exogamous and intermarrying classes was introduced for the purpose of preventing the marriage of brothers with sisters."

Leaving aside for a moment the concluding sentence of this judgment with the remark that it might just as well be argued that the *Table of Affinities* was introduced to prevent the marriage of a deceased wife's sister to her brother-in-law, and noting that abstract ideas, such as incest, must follow, not precede, practice grown into rigid "moral" custom, it may be remarked that if the inheritance of Mendelian characters follows exactly upon the laws of probability, we may reasonably assume that physical laws, however altered from their primal simplicity, rule in all departments or planes of life. The words "deliberate and repeated" in the above paragraph certainly call for scrutiny. As we observe that all political integration, or its opposite, follows inevitably upon causes which can be analysed into physical elements, racial, geographical, or economic, and that the element of deliberate purpose imagined to exist in politicians is the final result of the thrust of the energy behind them which they voice, it seems hardly likely that such action can be attributed to the prehistoric tribes of Australia. As I happen to know them I am far from underrating their intelligence, which, taking into consideration their conditions, is far higher than is generally supposed; but to believe them capable of performing such a moral and political feat as Frazer suggests is to outrage all probability. Communities are certainly not like crystals, and the importation of such a simile is in the nature of a rhetorical argument, which assumes in an opponent opinions to which he would never subscribe. But quite independent of any conclusion which, as its basis, takes for granted the facts of division, and then argues that it must have been deliberate, there is the ignored hypothesis that division never took place at all, but that what did occur was aggregation or integration. Since I put aside as untenable on the face of it, in view of our knowledge of the working of the human brain shown in the descent and progression of abstract notions, the theory that incest, as horrible or even undesirable on some real or fancied ground of eugenics or innate morality, can have anything to do with such phenomena, and that therefore division is much more than exceedingly unlikely, we are forced to consider whether, on a totally opposite hypothesis, political integration did not take place for reasons which may be discovered, or at least suggested, by considering facts it explains, or by parallels in the history of our own or other countries. Such facts and such parallels are easily discoverable, and as regards the last any European war has shown that pressure of circumstances tends to integration, often temporary, but sometimes permanent. An alliance, in the face of danger between races often deeply opposed through the operation of racial, geographical, or economic factors, if both are exposed to a common danger, is obviously of common occurrence. In such conditions ancient differences are hastily composed, compromised, or postponed, and a united front is shown to the enemy. These facts are too common and intelligible to need insistence; but it may be pointed out that on final analysis the like are in all essentials exhibited in the enforced behaviour of animals. The dogs of a village which are usually hostile to each other will unite to attack an invading dog. Groups of cattle which never graze together will ring round a centre and oppose together a prowling beast of prey. Many more instances might be cited; but the study of history itself is more than sufficient to show that union is never voluntary, but always enforced, while the fundamental hostility of groups is still seen even in English village communities, which fight when they meet, or enliven their hours of ease with jests at the expense of their neighbours, though they become, upon national stress, patriots and friends. But as there is no need to labour this point, it may be asked whether the hypothesis that the classificatory system of relationship and marriage, remnants of which are visible in all nations, cannot be explained by enforced integration rather than by division. Such an explanation will enable us to understand and classify many hitherto inexplicable and obscure phenomena in tribal organization, custom, and morality.

Since anthropologists, or those interested in anthropology, must be more or less familiar with the facts of cannibalism, there is no need to enter into a prolonged enumeration of its phenomena which are not yet co-ordinated. In preference to otiose repetition I shall therefore examine a few of the more remarkable customs connected with anthropophagism, for, if any solution can be obtained of them, many, if not all, of the remaining details will fall into their place automatically. Premising, then, that it is admitted there is sufficient evidence to assume provisionally that cannibalism is a stage through which early mankind has passed, it may be asked how it is that in certain Australian tribes, such as the Binbinga, the two classes eat, not their own, but each other's dead. In many other cases, according to Spencer and Gillen, it is suspected that the same custom obtains. It seems, however, as might have been prophesied, that those who are eaten are never, or very rarely, of the same totems as those who eat them. Such a custom is totally unintelligible on the division theory, and remains, as it were, a mere morbid degeneration such as some see in all cannibalism. Although many variations in man-eating must inevitably occur as totemism and tribal organization decay, what is to be sought is the origin of this peculiar custom, and if any hypothesis suggests it, the explanation should have solid foundations.

If the original patriarchal family was such, or nearly such, as Atkinson pictured it, and as I myself drew it, it can be seen how the single one-class, and single-totemed, group came into being. As said before, there is no need to follow Atkinson in his hypothesis that maternal love overcame the hostility of the brutal father and chief, for in such times and conditions as those which made the environment of nascent mankind, it is safer to infer that the changes were the result of stress of circumstance rather than of instincts, however beautiful, which in all primitive peoples tend to exert less and less power as the offspring become able to take care of themselves. In conditions, which even for the partially protected female were such as must have employed all her energy to live, they would have had little force.

Although the explanation of the past by what occurs now is sound in all sciences, such an instrument of discovery, when it uses not permanent physical causes, like those seen in geology, but evolved and evolving multiform factors such as the instincts, needs some caution. While the purely self-regarding and brutal instincts still seen in many must even so be regarded as modified favourably during social evolution, it may be assumed that the more altruistic were in their origin less worthy of admiration than they seem now. It is therefore more probable that outside stress, rather than maternally introduced modifications, conditioned the changes by which the sons were permitted to stay in some sort of growing communion with their parents, although avoidance was still strict, and exogamy, or marriage by capture, from other nascent groups or tribes, absolutely obligatory.

With the existence of families in such proximity as permitted wife capture it follows that, owing to the very custom of capture, some kind of relationship should have grown up. Even if the capture of wives were associated at other times with the capture of prisoners, or carrying off the dead for food, there would assuredly be intervals of peace and comparative amity in seasons of plentiful game. Although such friendly relations must have been slight they would certainly be stronger than those with other and remoter groups with whom no intermarriage was possible, and who were at all times regarded purely as enemies. It is very interesting to note that though Maine obviously knew nothing of the Australian classificatory system he yet remarks: "The history of political ideas begins, in fact, with the assumption that kinship in blood is the sole possible ground of community in political functions; nor is there any of those subversions of feeling which we term emphatically revolutions, so startling and so complete as the change which is accomplished when some other principle, such as that, for instance, of local contiguity, establishes itself for the first time as a basis of common political action. . . . The earliest and most extensively employed of legal fictions was that which permitted family relations to be established artificially, and there is none to which I conceive mankind to be more deeply indebted" (H. S. Maine, *Ancient Law*, 1905, pp. 129, 130). It may be noted, of course, that the two groups, which I conceive as becoming one tribe under external pressure, are locally contiguous and obviously blood relations, however bitterly hostile. If during a period of scarcity, distant and dreaded foes made an incursion into territory in which the less hostile groups were situated, there would be the strongest motive possible for at least a temporary alliance. If it is objected that the inter-group relations of marriage would not compensate for the hatred aroused by cannibalism, it must be remembered, *ex hypothesi*, that this was a state of things perfectly customary, and not in any sense outrageous. There was then no such thing as a moral horror of the practice. If such an alliance took place, and was partially successful, it would tend to continue, especially if the remodelling stress of the more hated foe still remained as fear "in being." It is natural enough to deduce from this that such a state of things would necessarily alter cannibalism between the allied groups, while necessity, combined with customary habit, would end in the practice of eating each other's dead, rather than in a continuance of former customs. On this hypothesis such habits as those of the Binbinga are perfectly intelligible; it is seen how they came about, and, by customary inertia, were continued. Although there is no necessity to enter into the vexed question of totemism, it may be assumed that in each familial group there was already some such name badge, whatever its origin and whatever it may have developed into later, when magic, myth, and tradition moulded and welded the tribe into its later form. There would be then a tribe of two classes, mutually exogamous, each class with a totem. And if such a hypothesis is admitted as possible, then on further pressure of war, induced as war practically always is by economic conditions, however much disguised in modern times, the transition from a two-class tribe to a four- or eight-class tribe actually explains itself. There would be in the latter case eight totem classes, whose ancient law sanctioned intermarriage with one other totem class only, while any intercourse between the rest of the classes, though at first conditioned mainly by jealousy, would gradually become tribal morality, and a safeguard to tribal unity.

On this hypothesis there is no compulsion to posit advanced abstract notions as a driving force, a conception contrary to logic and the nature of language as well as to the established fact that progression is from the concrete to the abstract, and not *vice versa*. Moreover such a theory, for if it unravels the meaning of so difficult a case as that of the Binbinga it becomes more than a hypothesis, is in accordance with processes to-day, while it explains modern sex morality as the last, but assuredly not enduring, remains of a tremendous and rigid code whose sanction was not merely the fear of slander or social ostracism, but that of death.

However disagreeable the conclusion may seem to many, even the positive evidence obtainable is suggestive of universal cannibalism. The fact that it still exists in many quarters of the globe, and may be returned to anywhere under stress, vastly strengthens such evidence. Abhorrent as it is to the modern mind, no one knows what he will do on occasions of which he has no experience. I once camped in the Selkirks, in British Columbia, with an old prospector known as the Man-Eater, because, when snowed-up and starving in the mountains, he had dug up his partner's frozen body, which he had previously buried. The potential cannibal may in fact exist in the most refined, and it is not illegitimate to conclude that the habit was once universal, and resulted in continued economic war. To such factors may be attributed the continual coalescence of many hostile tribes, who compromised again and yet again with enemy after enemy, and in the process established the earlier real societies, the germs of nations and of races. The unknown is still the horrible, and it was better to make friends with tribes near at hand whose customs were at least familiar, than to be conquered by dreadful far-off people, who might overthrow law and lay morality in the dust. What morals, indeed, were to be expected from such ? Magic itself might be in danger !

From a purely physiological point of view it may be suggested that as character is modified or intensified by special foods, containing special catalysts or toxins which act as determinants, a custom like cannibalism may have altered human character itself by ensuring a common average stock of such determining elements. We are even yet wholly ignorant of the evolutionary effects of acute diseases which are recovered from; but it cannot be imagined they have none. Their toxins by eventually producing immunity must certainly be a factor of change in a race, as they must be factors in the after life of the individual. It may be said that when tuberculosis was first active as a destructive and modifying agent, probably at the beginning of the pastoral ages when man first domesticated cattle, a very powerful factor of physical and cerebral change was introduced. If that is even remotely possible, it cannot be believed that cannibalism had not many obscure side effects, over and above that of intensifying the struggle for existence.

Organized war in itself, though it is a subject which has employed so many minds, has rarely been considered as a very ancient factor in evolution. We may take it for granted that it did not originate in the mere love of fighting. The joy of conflict is assuredly a by-product of superfluous energy, and even now much rarer than is assumed in romance. Yet if the temporary sullen alliance of some early prehistoric men, in periods possibly late Miocene in date, was the very beginning of tribal unity, and if their joint efforts procured success against a common enemy of both, we are entitled to call such an expedition the very beginning of organized war, as distinguished from solitary hunting, and the origin of immense evolutionary changes. So far as I am aware, though man's intellectual advance has been frequently attributed to his hunting proclivities, as calling forth qualities advantageous to him who was successful, and thus aiding his survival, while inter-tribal warfare is no doubt recognized as a factor of his progress, the reasons usually assigned for such warfare are the tolerably obvious ones still displayed among savage peoples as regards hunting areas. A French writer, Toussenel, who has discoursed on the part played by dogs as co-hunters with man, has, indeed, attributed the origin of cannibalism to a by-effect of the chase. He says (*L'Esprit des Bêtes*, 1847): "Il est évident que l'anthropophagie est née d'une excessive fringale combinée avec l'habitude du régime du viande. Il arriva que deux hordes de chasseurs se recontrerent à la poursuite du même animal, un jour que la proie était rare and que la faim mugissait dans leur entrailles, et il eut guerre entre elles. On se battit, on se tua et les cadavres de vaincus remplacèrent naturellement au foyers des vainqueurs les cadavres du gibier absent." Such opinions may, perhaps, be somewhat suggestive, but the attribution of cannibalism to the merest accident is most certainly not sound. Toussenel thought that it followed tribal organization and hunting in parties; but such organization has to be accounted for, not assumed as natural. Nor can we take it for granted that warfare arose of itself without some very definite and powerful cause among our very early ancestors, for though social animals fight among themselves, they never organize against other groups of the same species. Real organization for warfare, therefore, seems peculiar to man and some ants who have reached a very high stage as societies with great differentiation of function. Although imagination has a great part to play in speculation, when explanation is wanting and data are necessarily few such hypotheses as are invented must at least account for fundamental facts, and the view that tribal organization preceded cannibalism practically leaves out of account such phenomena as those of the Binbinga and others, which were probably as unknown to Toussenel as they seem to be to many modern writers on allied subjects.

It has been argued by Harry Campbell and others, as I myself argued twenty-five years ago, that inter-tribal warfare, in which all members of the tribe were engaged, must have meant the rapid elimination of fools and the unfit; but so far it seems that it has not been pointed out that the acts of war, whether tactical or strategic, tend to develop all the logical and mental faculties of man. Every human faculty has assuredly been called into existence by the stimulation of the environment. It does not follow that every stimulation has brought out power to deal directly with the situation. But it has certainly developed powers of avoiding possible evil results, thus moulding the race in another way. Certainly no faculty can be conceived existing without need unless there is in existence excess energy not wanted for self- or race-preservation. For there are undoubtedly developments not in themselves really useful. We cannot logically attribute the gorgeous colouring of the Trochilidæ purely to sex selection, when we see the less gorgeous species still highly adorned. We are forced to look on such super-coloration as the result of excess energy in birds already so energetic as to be almost beyond the ordinary accidents of bird life, while they live on the most assimilable form of carbohydrate. Such excess of adornment may in the end become harmful to them, as it has done since debauched destructive energy in rich women has demanded their sacrifice for adornment. But if all useful faculties in man, or any animal, must have been developed by the stimulation of the environment we can, I think, conceive no such stimulation as that of individual, group, and tribal warfare for the purpose of obtaining the food which is the most nourishing, and the most easily digestible of all. Since it has already been humanized, it calls for less energy to transform it into body-building or energizing factors. The very belief that eating other warriors gave the victorious their qualities, may easily enough have developed from the fact that such a meal showed a marked difference in results from those experienced with other foods. But the main point to bear in mind is that warfare with objects of this kind in view must have had not only very definite results in cerebral growth, but also very rapid ones. After a long period in which man was, perhaps, little more developed than *Pithecantheopus erectus*, such enforced organization, in groups which could develop subordination, and respect for ability in leaders, with the rapid concomitant destruction of less plastic anthropoid stocks of all kinds, must have resulted not only in the elimination of most of the ground apes, while the monkeys were able to preserve themselves, but also in a period of rapid progression in adaptability and cerebral development. Races may have arisen perfectly capable of slow progression to a status even higher than that of modern man, but if they lacked the enforced cohesion of those who had eaten up their hunting areas, and were finally driven into united internecine warfare to obtain food, or prisoners who could be slaves or food as necessity dictated, they would have had no more chance against intrusive voracious hordes with gross simian characteristics than Greece had against the armies of Philip. There are even suggestions in what we know of early human history which point, however vaguely, towards such unrecorded tragedies.

Among them, perhaps, may be reckoned that of the disappearance of such a species of humanity as Neanderthal man. His brain capacity of about 1600 c.c., while that of modern man is, say, 1400 c.c., is even more superior than it seems to that of the average European of the present time, as his average height was less (5′ 4″). His inferior stature may, perhaps, have been compensated for by greater weight, so that the portion of the brain devoted entirely to bodily functions, and not to intellectual qualities, may be rather more than that indicated. Yet here was a species of man, distinct from our own type by the possession of simian characters which are not ours, and the absence of some which we still retain, who had a greater brain than the average modern man. According to Keith, the teeth of this species are of the taurodont type seen in herbivorous or graminivorous animals. "On the evidence of the teeth and palate one is inclined to regard Neanderthal man as specially adapted to live on a rough vegetable diet. . . . His skill as a flint artizan shows that his abilities were not of a low order. He had fire at his command, he buried his dead, he had a distinctive and highly evolved form of culture." In spite of this culture, and the structure of the teeth, it is said that he was also a hunter, which is held to be proved by the remains found in the Krapina cave. He has indeed been accused of cannibalism, as split human bones were found with other scattered Neanderthal bones and teeth. The evidence is assuredly not altogether convincing, but it excites speculation, especially as the implements, though not typically Mousterian, certainly suggest that culture. May it not be said that if cannibalism there was, it must be ascribed, not to a typically vegetarian race, but to the contemporary ancestors of modern man, who have descendants practising it? In any case, whether such a hypothesis is regarded as mere fancy or real suggestion, the fact remains that a powerful and highly cultured race, which was obviously graminivorous and had probably reached the agricultural stage, with a cranial capacity not possessed by every philosopher of the present day, has vanished from the face of the earth, leaving no descendants and few traces of his existence. Such a problem cannot be disposed of easily. At the least it must excite the suspicion that his place was occupied by the ancestors of those who at one stage of civilization practised cannibalism, and in all devoted infinite energy to organized warfare. Vegetarianism is not likely to have been practised by those with teeth not peculiarly or typically adapted to such food, and it therefore seems quite possible that Neanderthal man was wiped out by swarms of a less advanced but more military race of cannibals. These speculations have at any rate the support afforded by such factors of evolution being in action even now, and if the hypothesis is correct, they must have been operating from a date some time after the anthropoid stock had divided into the lost species and that which even yet exists and, in many parts of the earth, still indulges in man-eating.

When considering the past effects of war upon the human races it may be urged that the typical soldier, even now, is the finest type of all-round man. This will no doubt seem a hard saying to those morbid intellectuals who overrate conscious mentation. Nevertheless, many who are prejudiced by the possession of an under-exercised body, and an over-exercised cerebral cortex, will probably agree that the all-round type of able and athletic man is the finest form of humanity. Not a few of those who belong to the higher intellectual order must often lament their own overgrowths and correlated incapacities when they contemplate his simple, healthy, and beautiful efficiency. It may be true that progress did not stop when fighting ceased to be the greatest factor in evolution, although, judging from the evidence it looks as if changes were more socially structural than cerebral. Yet even now we can say that the best officers in a good modern army belong to a fine order of compact, sufficient intellect. War still requires that equal balance of the body and brain which characterizes them, although they may want some qualities which in themselves are scarcely more than prophetic of possible future race characteristics.

It is not necessary to go deeply into cerebral physiology or psychology to see that war required the development of all the main faculties characterizing the human brain. There is no common faculty useful in life which is not necessary to the soldier considered only as such. War is a great intellectual and bodily game, in which the incomplete man goes under. The soldier has to reason, and must reason rapidly, his intuitions must outrun the processes of formal thought. To say so by no means implies that he should be acquainted with the syllogistic skeleton of human reasoning. Men argued in natural moods before scholastic logic, as they still argue without having heard of it. The early warriors organized brain tracts which grasped more and more factors in the environment; their skill and their salvation depended on new and ready response to new or old stimuli. Such brain faculties are capable of being developed and organized in a measure by hunting, but hunting alone, as we understand it, could not have put the last fine edge upon the brain as a weapon, nor would it have eliminated with any rapidity the small-brained races which were incapable of swift variation. Nothing but war would have been so likely to bring out all those qualities which reward skill, quickness, endurance, foresight, and the concentration of endeavour with the crown of victory and the inheritance of the earth. Thus it does not seem a vain imagination or a mere unsupported hypothesis to consider the early warrior's brain as the type from which all our still unstable new developments have naturally grown. The savage who was most a savage, who was the fiercest, the most ruthless, who was most endowed with cunning, and who yet had a faint sense of loyalty within his brain, which made him capable of being led or leading in his turn, was the true fountain of progress, of knowledge, and ultimately of those finer cortical growths which some metaphysicians and all religionists prefer to call "the soul." We must look to the lowest man-eating tribes who yet remain if we wish to see ourselves somewhat as we were when mankind first rose from the Miocene abyss.

Though these conclusions are disagreeable to many, while others think that it is straining an hypothesis beyond the limits of elasticity to reach them, it must be remembered that like objections are still urged against conclusions, as to the formation of human individual and racial cerebral characteristics, reached in psycho-analysis. These militate against curious concepts, such as Free Will, which are peculiarly dear to many, while they show that the origin of some of the sublimer feelings lies deep in the savage passions of self-regarding instincts. To attribute everything to cannibalism, without complete analysis of the way it operated, would be indeed a strain, but if it can be shown with plausibility that out of the practice of war there sprang, whether on Darwinian grounds purely, or on those which suggest that direct environmental adaptation can be inherited, such higher attributes of man as foresight, caution, subordination, respect for leadership, and other's mental endowments, while the whole basis of the organizing tribes substituted reason, which is the power of balancing possibilities, for purely savage instinctive action, we have a right, not only to conclude that cannibalism was an immense, even the greatest, factor of early evolution, but that we are no more justified in regarding it with peculiar disfavour than if we discovered with horror that the singular energy in doing good of some saintly woman had its origin in frustrate, sublimated sexual passion.

Moreover, if the conclusions arrived at in the chapter on the function of *Repair in Evolution* have any weight, we are forced to assume that such phenomena as failure and repair leading to favourable variation must occur in the realm of anthropology as well as in pathology and physiology. The more the theory is examined the more universal will be seen its operation, so that at last it seems legitimate to draw the conclusion that physiology in the sense of perfected action and reaction is an ideal of living structure, and no sooner seen that lost, while a morbid or semi-morbid condition due to over-stress and the reactions of repair, is the true norm in evolution. If this is so, and few capable of taking a scientific and philosophical view of society as now seen in the melting-pot of change, disaster, repair, and again disaster and new trials and errors as modifications take place under internal and external stresses and stimuli, will be found to deny it, we may take it for granted that the still vaster modifications of various species of humanity in the ages of geologic time must have exhibited like phenomena on a lower plane, in which the furious self-regarding instincts had not yet been changed, or only partly changed, into some of the higher attributes of man. When in imagination we regard such possible factors at work, the picture seems one of unmitigated misery, or social disease and disorder. Yet even then there were working factors making blindly for balance and symmetry, for easy action rather than difficult, for peace rather than war. Indeed, the most appalling comment to be made on such a state of nascent society is not that it was so peculiarly dreadful, or that it puts a strain on the imagination to conceive it, but that after long ages we see similar factors but little altered still at their work. Peace conferences have their ancient analogues and, as great diplomatists argued round Pliocene camp-fires, so, when Paris itself lies under the sea, other diplomatists will even then debate on ancient premisses, while idealistic, contemporary historians throw doubts on the recorded savagery of extinct Europeans.

If the whole of this volume were not in the nature of a plea for the use of the imagination in science, so long as it is controlled by ascertained results in allied branches of learning, I might have hesitated to use such arguments or illustrations. But when there are problems to solve, in which few if any direct observations can be made, and in which documents are rare, it is necessary to employ some such method as that known in mathematics as the Inverse Problem of Perturbations. Uninterpreted alterations in the orbit of one or more planets lead to the discovery of another almost beyond the reach of the telescope. If, indeed, Neptune had never been seen, the facts as to its orbit and distance from the sun would have been almost certain. Such a case presents striking analogies with investigation into prehistoric times. We observe the inexplicable present, and infer an adequate cause. If the present view suggested no more than a possible explanation of the remarkable change from such as *Pithecanthropus* to the modern type of brain which, following Keith, I believe to be of very early origin, it would, at least be something. Yet Keith himself says: "Can we conceive that, in the stretch of time between the end of the Pliocene and the middle of the Pleistocene, even allowing two or three hundred thousand years for that space, the brain of *Pithecanthropus* could have evolved into the modern human form? I cannot conceive such a rapid rate of evolution." While by no means of the opinion that *Pithecanthropus* was a human ancestor, for it appears far more likely that he was a collateral survival if properly dated, it seems to me that by the operation of the combined factors suggested such a rapid change might, and indeed must, have taken place. If there is one thing more sure than another, it is that stability of type indicates a more or less stable environment. From the historic view changes may be rapid, while from the physiological and anthropological standpoint they seem too negligible to be considered moulding influences. That very ancient types still survive is not really a relevant argument against a rapid critical period of change, unless we can show that such a static condition has continued through immense physical changes of the environment. The partially obsolete Darwinian view of a slow aggregation of minute advantageous spontaneous variations seems still partly responsible for the opinion that change must necessarily be slow. But in many states of matter they are often rapid, and it cannot be shown definitely that evolution is steady and continuous. Like the colloids of protoplasm, on which all life finally depends, it seems to have critical periods. Colloidal substances are easily influenced by obscure stimuli. The origin of life as a sudden rise in the organization of matter may have depended on a particular instance of ionization or the powerful influence of a rare accidental catalyst. Planck's very theory of *Quanta* itself suggests sudden steps in all phenomena whatsoever, and in the presence of such a protoplasmic cerebral tool, or catalyst, as the early discovery of cannibalism, I find no difficulty whatever in considering it as the last great cause of a sudden critical change in man. Alien as such methods of thought may seem to pure specialists in anthropology, they may prove suggestive to those of the opinion that analogous phenomena are found in all planes of evolutionary progress. Universal cannibalism must at the least be accepted, if accepted at all, as a possible instrument of rapid critical change, seeing that both as an eliminant and an integrator no more powerful machinery can be imagined. To say that its discovery as a motive for tribal integration may have been the work of some solitary old male genius, or the visionary glimpse, by some extruded exceptionally endowed youth, of a means of common safety, imparted by him to his young brother and thence to his mother who urged it on her man whose savage passions were already failing, may seem extravagant, but the notion will not appear so absurd if we remember that the thought of a relatively lofty brain is often the heritage of the best in succeeding generations, the common property of the herd in those that succeed, and that in the end it may be indistingishable from criminal and atavistic concepts. It may, and must, have been an infraction of custom, but, though for the ordinary man in any era there is little to choose between the habitual criminal and the habitual genius, necessity reinforced the suggestion, and made havoc of established law. Yet such new co-ordination would not be carried to its logical conclusion without the revolt of the more conservative element. It is, indeed, a peculiar and somewhat melancholy commentary on the perpetually recurring phenomena of social and human advance to view in imagination many a rigid and ethically-set incestuous solitary male retiring indignantly before the flood of immoral innovation into the darkest backwoods of the primeval forest. Had he been capable of such reasoning, he would have regarded the processes which led to progress and the evolution of the brain man now possesses as essentially anarchic, morbid, and diseased, just as the over-conservative mind of modern times regards the rise of new powers and processes in social polity as tending to the death of the organism of which it is a static and satisfied part. It is, then, no fanciful analogy which suggests that politics are but a chapter in anthropology, and that the processes seen in both are mutually illuminating. We may infer that as such new forces exhaust themselves in altered or adapted or entirely new structures, which in their turn must pass away, cannibalism itself died out among the races we call civilized when organization had reached such a pitch that the labours of pastoralism or agriculture promised earlier and better results than predatory war. A balance of power, continually upset and restored, came into existence, and the developing germ of international law or custom took on new forms. We can thus conceive Grotius and his followers as the lineal descendants of the first ancient inter-group messengers, or at the least derived from the calmer philosophers at the first peace conference ever held about some long-extinguished camp-fire over which the retreating ice of successive glacial epochs has poured its floods.

It seems not altogether impossible that this hypothesis may be confirmed by the aid of another branch of science. Some years ago it was suggested by an eminent zoologist, one of the few to whom knowledge has not meant specialism, that the evolution of *Tænia solium* supported such views. Since it can hardly be supposed that this parasite has reached its present perfect adaptation within the period which has elapsed since the domestication of the pig, which implies a settled or, certainly, less migratory state of civilization, it follows that the cystic form of *Tænia* must have alternated with the adult form in the one other form of life in which such a stage is possible, that is to say, in man himself. As I have not applied myself to the study of helminthology it was impossible for me to say whether this view was sound or not, and as the advocate of the hypothesis did not reprint the paper, I had an application made to him for his considered opinion. When this was not vouchsafed I requested information from a well-known helminthologist, who replied that he himself knew nothing of the subject, and therefore referred me to another authority, who in his turn gave me the name of yet another, who finally referred me to the first. It seems that, whatever is known of these parasites, their evolution has not yet been properly considered, and it may yet appear that it proves the universal and long-continued practice of anthropophagy.

On recapitulating the arguments advanced, though each one separately may appear unconvincing and even capable of rebuttal, it seems that when viewed together they amount to much more than might have been expected. Even a partial enumeration of the points discussed may suggest reasonable explanation of the following difficulties :

1. The classificatory system of relationship.

2. The custom of avoidance and the sex-coldness among brothers and sisters.

3. The not uncommon modern phenomenon of parental jealousy.

4. The widespread modern practice of cannibalism in all its various forms, food-seeking, honorific, and religious, etc. etc.

5. The peculiar practice of endo-anthropophagy which consists in each class of a tribe eating the dead of the other class.

6. The rapid change in cranial form and capacity during a comparatively short time.

7. The development of the basal logical faculties of man.

8. The disappearance of a big-brained and probably agricultural species of humanity such as Neanderthal man, and the facts observed at Krapina.

9. The evolutionary functions of war.

10. The nature of hidden racial complexes strictly analogous with those observed in individuals.

REFERENCES.

CAMPBELL, HARRY.—"Biological Aspects of Warfare," *Lancet*, London, Sept. 15, 1917; and "Man's Mental Evolution, Past and Future," *ibid.*, London, 1913.

FRASER, Sir J. G.—"Totemism and Exogamy."

KEITH, ARTHUR.—"Antiquity of Man," 1916.

MAINE, Sir H. S.—"Ancient Law" (ed. Pollock), 1903.

SPENCER and GILLEN.—"Native Tribes of Central Australia."

TOUSSENEL.—"L'Esprit des Bêtes," 1847.

CHAPTER VII

Heredity and Environment

IN science one of the most successful Teutonic warriors appears to be Weismann, who imposed his yoke on the larger part of the biological world. They would still seem happy under it, even if uneasy at times, and, perhaps, more doubtful than they appear. It is the duty of the orthodox to disclaim doubt and to profess belief with fervency. This is especially binding upon those who occupy the pulpit: if the priests of neo-Darwinians, that cult purified of pangenes, use, and disuse, and the transmission of acquired characteristics, showed hesitation and ceased to preach dogmatics, their reputations would be ruined, and the congregation become a lost flock. Too little stress is laid on the vices of orthodoxy for, not only does it make men blind, it makes them cling to untenable positions. It would be more than terrible to discover that theirs was the worship of no translated but a vanished god. For did not Zeus himself die, and is he not buried in Crete?

I do not propose in a short chapter to deal with the whole case for the transmission of acquired or altered characteristics, either on its theoretic or experimental side. But it has already been suggested in this book that to neglect relative speculation, that is to say, speculation dealing with like phenomena on different planes of life, is to put aside a powerful weapon of analysis, and there are, or so it seems to me, reasons for believing that some purely theoretic criticisms of the germ-plasm hypothesis may help to show where it is true and where false. In any case the orthodox can hardly complain of the use of theory since Weismannism, however its characteristics have been altered and transmitted, is still almost purely theoretical, being supported chiefly by the argument that no other view accounts for everything. This is, however, a theological rather than a scientific argument, for the inclusive and complete hypothesis is dear to the ecclesiastic mind.

It can, perhaps, be remarked that orthodox biologists do not avail themselves of all biological resources. In discussion the salient fact emerges that they rely mainly on cytology for practical support. But since cytology is dependent on the microscope, a valuable but increasingly hazardous tool of research as higher powers are used, the more observations are extended the more uncertain are the conclusions reached. Among few of the pure school of neo-Darwinians do we see the biological conception of the organism properly considered, nor do the devotees of cell-structure and the ever-enlarging ritual of the chromosomes seem to reflect that every cell they observe *in situ* or in the dark field is after all a unicellular organism. When it is so considered, since any organism is a definite spatially related set of colonial organisms, it might even seem that Weismann himself had given his whole case away by admitting that unicellular organisms could and did acquire and transmit acquired or altered characteristics.

It may be repeated, moreover, that biologists however learned in cytology and the pure literature of their own subject, for the most part ignore all the related sciences. With regard to pathology I have endeavoured in some measure to make good this omission in the remarks on *Repair in Evolution,* and although it is obvious that the conclusions reached there are not likely to be greeted with enthusiasm by those who hold the germ-plasm theory, I shall not now lay any great stress upon them. In this place it may be more pertinent to turn to general histology, a subject which so far seems little known to those engaged in biological study. For nearly all work upon heredity appears to begin with, and to be founded upon, a consideration of the perfect gametes, and to proceed with elaborate accounts of their reduction, maturation, and fusion in the zygote without taking into full account the tissue history of the organs in which they arise. In saying so much the insistence on germinal epithelium is not overlooked, for nowhere have I been able to discover why it is called "germinal," except from the fact that in the higher organisms the sperm and egg cells descend from epithelium. Although in many of the lower kinds they spring from blood cells, or other cells, this fact is interpreted by Weismannians as the pressing of germ cells into general service, a view which is an outrage on logic. It may be suggested that the tissue history of the colony of epithelial cells in which they develop must discover one very important fact, which is that they have a special environment, and that when they are "born," that is to say when they leave it for another, the second or third place they occupy, though still an environment, is less and less special as the growing cell itself specializes. It seems to be forgotten that among mammalians the offspring is at the least "born" three times, once when extruded from the Graafian follicle, once when it throws in its lot with the sperm cell and makes common stock of its energy and chromosomatic tools, and again when extruded by the uterus after a prolonged period of parasitism. With a properly and naturally nourished infant the number of births may be said to reach four when it is weaned. Far too much criticism is made of direct adaptation to environment in the adult organism, and far too little study given to pre-embryonic and embryonic stages, even by most of the advocates of such adaptation.

As mitigating to some extent the fairly obvious biological ignorance of histology, it must be admitted that very little seems known of the histology of the ovaries and the ova, the testes and the spermatoza, for Schäfer disposes of the subject in a few lines, and other authorities are equally brief. Something may be found in Wilson, and Weismann himself dealt with it ineffectively. As his theory rendered it unimportant, this is not a matter for wonder. The ovarian tissues and the history of the oöcytes seem less known than that of the testes, although in this last case much remains to be cleared up. It is a fact that both sperm cell and ovum develop, not from any more obviously special tissue than epithelium, but very often from epithelioid cells which have not taken on the full character of epithelium. To deal first with the testis, we may say with Schäfer and Brown, that the sperm cells are developed from the small spermatoblasts which form the inner stratum of the seminal epithelium, and that these themselves are formed by division from the spermatogenic or mother cell of the second layer. It seems probable that these descend from the lining epithelium. Thus we have a definite descent of the sperm cell in at least four stages: (1) Division of living epithelium cell into two cells, one of which becomes a spermatogen and passes into the second layer, while the other does not migrate, but enlarges and becomes a sustentacular cell, apparently connected with the nutrition of the spermatozoa when fully formed and during conversion. (2) Division of the spermatogen. (3) Further division and resulting daughter cells are converted into spermatoblasts. (4) Growth and elongation of spermatoblasts into spermatozoa.

In the ovaries similar processes appear to take place by which the follicles are developed from lining epithelium. Some of the cells develop into ova, and are thus direct descendants of epithelium. So far it seems that there is no reason whatsoever to be found in any of the processes for assuming that germ-plasm in the narrow sense exists at all. The succeeding phenomena can be accounted for without any great exercise of faith if we consider such processes as dependent on the cell's energy and the catalysts, or tools, brought over in the oöcyte and sperm cell, or derived later by the zygote from the tissues and blood-stream of the maternal parent. For during the most important part of the reproductive cell's life, that spent in the originating tissue, it was a unicellular organism acquiring the characteristics which under other conditions develop and diverge. If such a view is accepted the great determining period of the reproductive cell is its early testicular or ovarian history, not that of its later embryonic life. During the first state we can easily imagine the epithelioid cell acquiring freely the activators, catalysts, or similar hormones, which direct operations in the adult organism. To say that it contains germ-plasm is to assume something without real proof, and no observations of germ-tracks, or theories of germinal epithelium as ultimate facts, can invalidate the conclusion that, as the function and form of the adult are determined by definite agents, so the functions and form of the free oöcyte, sperm cell, or zygote, are thus determined from moment to moment of its development. Such a view takes into account the law of parsimony, which requires us to posit no unknown factors where known ones can be seen producing similar results.

Pure early theoretic Weismannism has no doubt been modified and diluted. So has early theology. We are no longer required to assent to a cloud of biophors, a hierarchy of determinants, and a whole angelology of ids as a *sine qua non* to biological salvation, though it may possibly be shown that the dilution of the theory has not allowed for the truth in it, if it is considered rather as an illustration than as true theory. But still sufficient of the suggested machinery remains to enable the neo-Darwinian to believe that all change is due to minute germinal variations in the chromosomes, though no one of them has yet acknowledged that such variations are variations in definite tools, as even Weismann himself might have acknowledged if as much had been known of the endocrines in his time as is known now. Disguise it as they may, the whole theory as held is concealed vitalism and a *circulus in definiendo.* If the germ-plasm is an ultimate fact not resolvable into recognizable scientific factors it is absurd to call the theory scientific, unless it is asserted that " nature " and " life " are scientific words instead of verbal shorthand. Nothing, indeed, can be described as scientific explanation which cannot in the end show phenomena as the result of known factors. Thus ultimate explanation is not explanation at all. We cannot yet resolve final physical laws, and therefore ultimate physics can only be called descriptive. It is true the phenomena may in the end be ranged under mathematical conceptions; but mathematical reasoning is not science in the strict sense. It is abstract illustration of theoretic possibilities, and thus akin to pure logic. The theory of Weismann, if it has any foundation, must be capable of resolution, and may not be looked on as a quasi-mathematical or purely verbal illustration of possible mechanism. The effort of the neo-Darwinians to dispense with his terminology is, indeed, not sound. What they should have done, and what remains to be done, is to see if his terms will bear translation into measurable factors. This, I think, can be achieved but, if it can, the "nature" of the germ-plasm will disappear and theoretic determinants must disclose themselves as hormones, enzymes, catalysts, and successively formed internal secretions, by which each early cell-change or later embryonic or adult development is actually determined. As held, the theory is but a form of the "Absolute" conditioned purely by natural selection. It may appeal to some philosophers, and to those whose tendency is to short-circuit explanation by the hasty use of final definitions; but it might at least give pause to its adherents to observe that anatomists, physiologists, palæontologists, and many others work habitually on the theory that, whatever the mechanism, modifications can be transmitted. It is true they may agree with the orthodox biologist that in such cases the nuclear contents of the reproductive cells are altered; but they would certainly add that such an alteration must be in the nature of an addition, subtraction, or new combination of substances of a catalytic or determining nature. And if this is correct it follows, from all we know of iron-using bacteria to the latest hypo- or hyperthyroidal patient, that these substances, however simple or complex, can be added or taken away, and that in the food, or in successive metabolic states resulting from its use, new catalysts may be formed, combined or changed, as they can be by environmental stimuli such as light. When saying so much it should be added that I am aware of the work done, which, in certain cases, shows, or seems to show, that there is an early isolation of a germ-cell, *ex hypothesi*, containing the unimpressionable "germ-plasm." Yet whatever may be found with regard to the embryo of a shark, or any of the cases held to prove such early specialization, the facts are insufficient on which to found a general law. They afford no explanation of budding or repair, or the cases in which "germ-cells" are wandering amœboid bodies, and even blood-cells, or of the so-called germinal epithelium itself. To speak, as is often done, of specificity of detail as being determined wholly by chromosomatic facts, without resolving the magic of "specificity" into definite "tools," is surely idle. It is concealed vitalism. Nor do we really learn much when we are told that in certain cases germ-cells do not arise from cœlomic epithelium, but that they migrate from special germ-areas into the gonad, since there are so many different ways in which such specialization begins.

To show that the natural tendency of the physiologist is to accept such a view as transmission Starling may be quoted. His work on hormones, done in conjunction with Bayliss, shows that he has a great appreciation of the power of certain secretions to influence in the profoundest degree digestive and metabolic processes. The possibility of prostate secretion tabloids curing chronic mastitis may be mentioned (Lane). Since few biologists concern themselves as much as they should with physiology, and not at all with pathology, which is just as necessary a part of their proper apparatus, it may be pointed out that some internal secretions have such observable effects in the minutest proportions. That it becomes intelligible how the minute parathyroids, four of which weigh two grains, have such great physiological effects as to make certain they are real determinants. According to Schäfer, a strip of intestinal muscle is affected by adrenalin in a solution of 1 in 20,000,000 and a strip of coronary artery by 1 in 50,000,000. Pysemsky and Kravkov state that the effect of one part in 250,000,000 could be detected when perfusing a rabbit's ear with Ringer's solution. Such results may at least suggest that an almost infinitely small proportion of an inorganic catalyst or organic secretion, whether coming over in an egg or sperm cell, or taken in later from the parental host, might be a determinant of immense capacity. No doubt such ideas as these moved Starling to the statement that " cell-division in the organism might be spoken of as the evolution of a new kind of cell, but that the change takes place within the development of the multicellular parent, or host, instead of occupying a long space of time and involving the destruction of countless individuals as when a change of type occurs gradually in a unicellular organism." Now, independent of the fact that we have no evidence that a unicellular organism may not change as quickly, or even more quickly, when transferred to water with different saline constituents, as an Alpine flower when transferred to the warm lowlands, and even positive evidence that it can so change (J. Loeb), it may be remarked that about seven months from impregnation is sufficient for a new human being to become viable. That delivery usually takes place at nine is due no doubt to the average size of the pelvic ring. Yet the maternal organism took many millions of years to become what it is now if some anthropologists are right in thinking man, as man, dates back at least 1,500,000 years. If the evolution of such a high metazoan from a unicellular organism took only ten million years, which seems an impossibly short time, similar changes are actually repeated in about six months, say $\frac{1}{20000000}$ of the time of evolution. On what grounds then can we assert that some undifferentiated protoplasmic units cannot become developed oöcytes during the time from birth to puberty? The simpler spermatozoa, also developing from unspecialized epithelioid tissue, come even earlier to maturity, as they may be found active in infants. Time does not seem the essence of the contract, for the whole physiological theory of living matter is practically based on what is known and measurable, the activating and accelerating qualities of catalysts. Without going to the philosophers or metaphysicians, to Kant or Einstein, for instruction as to the relativity or physical nature of the time concept, we can recognize that it is at least purely relative in physiology and biology. The whole of evolution, as of education, is the discovery of short cuts, and in this the *Principle of Least Action* is at work. Free energy perpetually adopts the shortest path to become bound. Common sense itself is that principle in social work. Little by little the organism as it evolved picked up and transmitted by successive experiment, by trial and error, activators which hastened processes. Time, therefore, in the sense that Starling used it, does not seem to be an essential factor. His instincts, and his knowledge of activating principles and processes, appear to have led him right after all. Activating elements are supplied fully developed by the parents who took uncounted ages to acquire them. There is no reason whatsoever for not endorsing Starling's almost wistful statement, although in some moment of doubt he rejected it at last. If the elements themselves show that in a like temperature-environment they stay the same, and change when it changes, and yet go back when it again alters, no more is asked by any advocate of transmission. We may even say, as I have suggested elsewhere,[1] that the whole course of evolution suggests that what we have to discover is not why child is like parent but why, in certain cases, it is unlike, being sure as we pass to the investigation that some internal or external environmental cause is at the bottom of the alteration. Belogolovy bred ova of the frog Pelobates in the parental body cavity. Their " determinants " determined nothing as the ova became parasitic and presented highly abnormal characteristics, not Pelobatic at all. Such considerations may no doubt be dismissed as purely theoretical, or even excursions of the fancy; but if it is noted that the greatest weakness to which all scientific men are liable is the natural tendency to take the easiest path, and ignore general principles, they may not seem so much out of place. The easiest path at a given time is not always the right one. We may get very doubtful adaptation to facts, for energy over the pyramidal tract does not work with the same certainty as adrenalin.

If the orthodox school could give any hint as to how a variation is to be explained, and what it is that is changed

[1] Vide *Repair in Evolution.*

in the germ-plasm or the chromosomes, they would be compelled to come down to the earth and stand on the firm ground of chemical or biochemical action. Once there they might be led to admit at last that any steady external stimulus may alter one cell, and that if so it may alter many, or that the accidental acquisition of some metal or salt may end in its being a permanent tool in the armoury of the whole organism. Their very insistence on germinal qualities and intra-germinal " struggle " and selection is sound so far as it goes; but they cannot be allowed to remain juggling with such factors without telling us in what the struggle consists, and what weapons or tools are used, or at the very least without taking into consideration what other sciences can supply them with. It is a sound principle, and certainly one I have always tried to bear in mind, that no body of earnest workers can be altogether wrong. Even the Hering-Semon " mnemes " and Samuel Butler's " memory " can now be translated into biochemical factors. If in one sense a " mneme " seemed to mean no more than that an altered thing was no longer what it was, we may still turn the word into measurable factors. The experience of the cell is in its education, its acquisition of new tools, and " memory " is but the due repetition of phenomena when like causes and catalysts are in action in like tissues. The desired bridge between those who assert and those who deny transmission must in the end be found by building on factors which admit the basal doctrines of both. It may be admitted that the " germ-plasm," or reproductive cell with all the tools in its nucleus or scattered as granules among the great society of its molecular units, changes for the most part with great difficulty. It is a conservative social organism. But change it does, and in the end changes must be due to the whole of the environment. Exactly the same may be said of the social organism. Few who take a philosophical view of history would deny that the most salient fact about man is really his conservatism. They might even adopt the terminology of the biologists, and say his germ-plasm altered not at all. Yet on further reflection they would admit that a similar "victory over nature," as occurs when a cell gets hold of a new tool, occurs when man learns to use steam or electricity. From one point of view, with a short time-element, social change seems rapid. From another it appears slow. We may say that any organ is elastic or rigid, just as we please, according to the point of view we happen to take at the time.

As I have pointed out in other places, the obscurity of cellular phenomena is probably greatly increased by the assumption that the nucleus is "alive," that is, composed of protoplasm.[1] There seems no evidence for this beyond the fact that it contains nucleins, the whole chemistry of which was worked out by Emil Fischer (Bayliss). These nucleins are compounds of a protein with nucleic acid. Many enzymes deal with their metabolism, and it is far more probable that they are the reserve food protein of the living protoplasm than part of that much more obscure and complex protein engine. Certainly it seems that a far clearer notion of a cell's activities is reached if we conceive it as a social aggregate of protoplasmic units, however complex they may be, with a storehouse of food and tools or working catalytic bodies, than if we regard the varying moving nucleus as a live part of it. When a test-bearing protozoon has

[1] Vide *Method in Science.*

its test pierced the nucleus moves up to the breach and repairs it. Such a process mimics purposed action, and, indeed, *is* purposed action if, as certainly may be done, we analyse all purpose into complexes of tropisms. The probable causes of the nuclear movements are the negative tropisms of the protoplasmic elements. They are repelled by the salts of the water, in which the cell lives, and from which the test protects them, and gradually thrust forward the non-living nucleus which contains the catalysts or tools which can hasten the deposition of such constituents of the cell wall as are needed for repair. The process is exactly similar to that of an expert with tools being thrust and drawn into the position in which he can use them to make good the result of some accident which requires instant attention. These views are supported by the work of Haberlandt.

This conception of a nuclear tool-house and storehouse brings the cell as a social organism into line with those we more commonly call social, and if the generalization is made that living action of all kinds, in the cell, a tissue, an organ, an animal, a social body, or an "individual" such as a nation, is of a like nature, it may be inferred that it is not so much on the actual protoplasm itself as on the acquired tools, and what is made by them, that differences of form and action depend. The same protoplasmic energy engine makes a muscle cell or a neuron. The notion of different kinds or grades of protoplasm appears to be without foundation. That of a sperm cell or a hepatic cell is probably just the same and, if Child (*Senescence and Rejuvenescence*) is right, it may surely be inferred that an increase of protoplasmic activity depends on new tools, the increase of old ones, or the loss of those once useful which have ceased to be so, while its decrease follows on the retention of what is no longer needed or effective—a highly conservative proceeding. The conservative tendency to retain property of all kinds is thus seen in the very cell, and a house crowded with useless lumber has its true analogue in a so-called senescent cell, which has become static and rigid with a morbid "sense of property." Old age is truly hindrance and poisoning, not necessarily any alteration of protoplasmic units, whatever they may be.

To some it may seem an unjustifiable inference, but the conclusions reached in this way tend to show that every determinant, late or early, is a definite tool or engine. England is not the same country that it was when wood was used instead of coal. It changed with great rapidity when the use of steam became common. Electricity has till greater possibilities of change. But we cannot assert that the brains of the modern business man are better than those of the Athenians, or that Watt and Stephenson were greater geniuses than men's early ancestor who first made a wheel, or the one who discovered that water poured on the early rude axle acted as a cooling agent and lubricant. The reason of the rapid advance of the Americans in material civilization was their adaptation of the English tools into an organism less cumbered with static elements. Vested interests discover themselves as slowers of metabolism, and as obstacles to new construction, the result of new tools which can be acquired and transmitted. Germ-plasm on this view is just the same as any other plasm, and if the neo-Darwinians insist that practically, that is, in any given time, it does not alter, no one will have any quarrel with them. But those who believe that cell is a social aggregate using tools as much as an animal or a society, and that the same laws rule all organic growth and change assuredly, cannot accept the view that Natural Selection and germinal accidents are the sole causes of variation. Such conclusions imply entirely different laws for similar aggregates, and have an unholy resemblance to vitalism, the conception of entelechies, or to Driesch's rudimentary psychoids, surely the most humorous extravagance since Hartzoeker's Homunculus.

It has not been my intention in this paper to point to the strong evidence in favour of transmission of acquisitions.[1] Cunningham, MacBride, Kammerer, and others can take care of themselves, and have presented many enigmas to those who would solve them on the principle of the continuity of germ-plasm. To those, however, who have read the chapter on *Repair in Evolution* it will be obvious that the evidence brought forward there must be rebutted, distorted, or rejected, without consideration of the general laws of mechanical or other construction, if the theory that variations are due to the phenomena of fertilization is to have the remotest chance of survival. I may, however, remark that further reading and consideration have confirmed me in the view that variational repair takes place during embryonic growth owing to increased functional activity due to relative changes of catalytic elements in the parent. To those with the smallest knowledge of histology the phenomena of muscle growth alone are sufficient to prove this, unless they are content to believe that small minute variation can construct such a wonderful though obviously repaired organ as the heart. Organ-forming substances there undoubtedly are, but they must finally be translated into chemical or biochemical agents, probably of a discoverable kind,

[1] See Appendix B. The *Peroneus Tertius*.

which influence all forms of growth. Thus even Lewis's experiment of transplanting the optic vesicle, with the result of a transformation of the skin above it into a rudimentary lens, will probably be finally explained as the evolutionary possession by the vesicle of a catalytic secretion activated by light which alters the form and structure of the epithelial cells in close contact with it. We can conceive no organ-forming substance to construct the heart; but it is easy enough to regard it as a progressively formed functional adaptation to stresses imposed upon it during embryonic growth in which it is, to use Starling's words, "a new creation." Von Nägeli and Hertwig pointed out with each stage in growth the internal environmental complexity increases. But such complexity uses an increasing complexity of tools, for just as mere increase of numbers in a factory without new instruments does not necessarily result in new differentiations among the workers or different structural developments in the buildings, so in the animal organism mere increase in bulk does not imply increasing complexity. The most important variables in all growth, structure, and function are the "tools" used, and the engines made of them, and the illustrations of the phenomena of budding and mitosis given in *Method in Science* are probably far more than illustrations of the way in which organisms in a changing environment acquire the tools which change function and change structure and can be transmitted, just as they can be lost in another environment.

It seems, then, as if Weismann occupies the position of a mathematician who works out a set of equations in which *a*, *b*, *x*, and *y* obviously represent no more than possible theoretic factors leading to a conclusion which is afterwards found to be near the mark as soon as the letters employed are translated into physical agents. So far Weismann was right. But living processes work out like complex mathematical equations. The Binomial Theorem may be in $(x+y)^n$ in one sense, but so was Keat's "Ode to the Nightingale" in the alphabet. It is common among mathematicians to say such and such an equation "naturally becomes" such and such, or takes another form from which yet another can be deduced. This "naturally becomes" is intelligible to another mathematician, but the unlearned require the insertion of the steps omitted to perceive that the change is logical. The orthodox theory omits the links, and does not turn its prime equation into things. There is a likeness between such algebraic processes, and those which take place in the living organism, for we find that if certain tools are used in ovarian or embryonic stages they "naturally become" varied in action, and though we may know little more of a chromosomatic tool than we do of a or b in the original equation, we perceive that in conjunction with other activators it changes into adrenalin, thyroidin, secretin, or some other regulative or directive hormone. Moreover, as in mathematical reasoning we may introduce a new variable while the constant remains the same, so it is with the organism. The constant is protoplasm. Not all organisms use iron. There are some which use manganese. At some period a descendant of the ancestral amœba of the mammal picked up iron and used it. It is employed in varying quantities. As evolution progressed internal secretion after internal secretion came into existence, determining living action. Without adrenalin the mammal could not meet danger quickly. But it is as absurd to argue that the mere potentiality of adrenalin is a determinant in the chromosome as it would be to argue that the possession of a fleet is determined by the "nature" of an embryonic tribe which has not yet seen the sea. Environment and function cannot be ignored. A fleet does not grow up by minute advantageous "spontaneous" variations. It is an acquired tool, and itself determines further historic evolution. The "constant" in germ-plasm is the nature of protoplasm: its infinite variability, as shown in all forms of life, is a variability which is further and further constricted into more and more definite lines by definite constructions, until at last in a static environment stasis is reached. Yet the discovery of another tool, a new means of short-circuiting labour, may again set the static organism upon a voyage of discovery among the potentialities of life. With change of function, which should include the phenomena of regeneration and reduction, comes morphallaxis. Without it there is none. Death itself is an acquired characteristic. If the organism were not perpetually preyed on by other organisms, which by parasitism and poisoning divert or hinder energy, inhibit, or over-activate, metabolism through the induction of changes in the endocrines, and destroy tissue functions generally, it is conceivable that such a characteristic as death might be lost, and that any body, however highly developed, might resume the long-abandoned characteristics of unicellular organisms, and again become practically immortal.

REFERENCES.

BAYLISS and STARLING.—"Mechanism of Pancreatic Secretion," *Journ. Physiol.*, London, p. 28.

BELOGOLOVY, G. A.—"Nouveaux Memoires," *Soc. Imp. Naturalistes de Moscow*, 1916.

BUTLER, SAMUEL.—"Unconscious Memory."

CHILD, C. M.—"Senescence and Rejuvenescence," Chicago, 1915.

FISCHER, EMIL.—"Untersuchingen in der Puringruppe, 1907; *v.* Bayliss, "Principles of General Physiology," 1915.

HABERLANDT, G.—"Ueber die Beziehungen zwischen Functionen und Lage des Zelkerns," 1877.

LANE, Sir A.—*Lancet*, London, Oct. 9, 1918.

LOEB, JACQUES.—"The Organism as a Whole," 1916.

OLIVER, G., and SCHÄFER, E. A.—"Physiological Effects of Extracts of Supra-renal Capsules," *Journ. Physiol.*, London, 1918.

PYSEMSKY and KRAVKOV.—"Adrenalin and Ear of Rabbit," *Russky Vratch*, vol. xi. p. 264.

SCHÄFER, E. A.—"The Endocrine Organs," 1916.

WEISMANN, A.—"The Germ Plasm," London, 1893.

WILSON.—"The Cell."

CHAPTER VIII

The Origin of Therapeutic Bathing[1]

THE comparative study of the sciences, upon which I have insisted, I trust not unduly, may not be only of value where pure science is concerned, but may also prove of immense service in many of the arts of life. The conception of hostile symbiosis is of such obvious relevance in politics that what was an art can at once be converted into a section of biology. Moreover, this and allied conceptions tending to show the vital analogies in all construction may be employed generally in education, and especially in medicine, in which narrowness of outlook is especially dangerous. For knowledge of one kind may, and indeed must, act as a catalyst on thought with regard to another. It seemed to me when first considering the subject of this chapter, which may, perhaps, seem not strictly connected with these that precede it, that anthropology, upon which light can be thrown by general biology, physiology and pathology, might prove of the greatest value, if taught intelligently and with due appreciation of its wide bearings, to all students of the human brain and body. It thus appeared to me that a very simple subject which was

[1] Although never read, this paper was written as an address to a Balneological Society, and therefore may retain some indication of its origin. For most of the facts I am, of course, indebted to *The Golden Bough*, the mightiest storehouse of co-ordinated knowledge in the English, or any other, language.

still obscure might illustrate this better than abstract reasoning, if the processes involved in its study showed in what ways the brain is apt to work, and how purely magical concepts may lead to useful discovery. If we learn how our remote ancestors thought, we shall discern, perhaps with humility, that we are their true descendants, and that modern life with all its advantages, even the modern balneologist and the very household bath itself, is still a subject for the anthropologist.

Although very few of us are like the Japanese maid who is said to have apologized to her European mistress for not taking more than three hot baths during a busy day, to most educated men bathing seems a natural, almost an instinctive process. They would be uncomfortable now if anything went wrong with the morning bath, as it is apt to do when the coal supply fails. Such discomfort, however, is soon cured by compulsory abstinence, for my own experience has taught me that after three days want of washing little discomfort is felt by the average man. On two occasions in my life, once at sea coming up to the Falkland Islands from the Horn in very heavy weather, and once in the Australian bush when there was a drought, I was unable for a fortnight at a time to do so much as wash my face. The feeling of discomfort disappeared on the second or third day, and I seemed ready to do without washing for the rest of my life.

The truth is that cleanliness is not natural to mankind. Most parents know from their own experience that to teach a child to persevere with soap and water is the most arduous task that falls to a mother or a nurse. Washing thus appears to be anything but the result of instinct, since it is not so much as an easily acquired habit. Unluckily for the vast body of the population in our civilization it is not even economically possible. Those who have read the books of George Gissing may remember that he answered the assertion that the poor might at least be clean by exclaiming, with bitter truth, that cleanliness was an expensive luxury. Among many of the agricultural and pastoral peasantry of Britain a man is washed all over twice, or at most three times, in his life: once when he is born, once when he is married, though this is not universal, and once when he is dead. Yet bathing before marriage in many cases is practically a magic ceremony, and since magic dates from the remotest period it might be supposed to remain as ritual. There is no doubt that the washing of children at birth was also anciently purificatory. The rôle that blood, especially the blood of women, has played in the history of lustration is very remarkable. That the corpse is also washed after death is, of course, also the remains of a ceremony of purification. But if it is a fact that washing in its origin was due to religion and magic, as seems certain, how did it begin at all? It may seem absurd to ask such a question; but the more we know of anthropology, which is but the study of man in the making, the more it is seen that all apparently natural processes must have had a beginning, and require an explanation. It has often been observed that even the instincts themselves are not perfect, and require experience and education. One of the very deepest and most ancient, that of sex, is certainly not least in need of it. One need not read Havelock Ellis to discover so much, seeing that many of those engaged in obstetrical practice have assisted at deliveries in which the infant in the act of birth destroyed the unbroken hymen. Education is not only needed with the sexual instinct; but, if Horace Fletcher and Doctor Chittenden and Sir Michael Foster were right, it is the same with some of the obscure reflexes connected with eating. The reflex which prevents, or should prevent premature deglutition, is outraged by all, especially when they enjoy the pleasures of conversation and the table at the same time. I do not know whether it has ever been noticed, even by the observers I have named, that this particular reflex only comes naturally into play when savage methods of feeding are indulged in, that is to say, when the mouth is crammed with food and swallowing in the ordinary sense is actually impossible. If we decide to re-educate this particular reflex Fletcher advised us to work over long paths to restore its efficacy; that is, to attend voluntarily to mastication. It may, however, be pointed out that if a child is not interfered with by a polite mother it will fill its mouth so full that deglutition without thorough mastication is impossible. The natural instinct will lead the child to use and preserve its teeth and its digestion. The pleasant, but physiologically damnable, habit of cheerful conversation at meal-times should be corrected. There are, however, no instincts which lead the young to bathing, and such reflex actions as are connected with it are, among the simple, merely those of repulsion. The instincts of mankind are really against it. What then was its origin, seeing the common dislike and even horror of water displayed by those unaccustomed to it, and the comparative ease with which even the most cleanly under pressure learn to do without it? I think it will not be so difficult to find how it arose and branched into purification and therapeutics if we delve into the far past with the help of anthropology.

The first thing that one learns in dealing with primitive man is, that although he was logical, his premisses partook of the simplicity seen in children, even the most intelligent, as they learn how to deal with the world before them. What seems perfectly natural now was by no means natural to primeval man. How indeed could it be when their great working hypothesis of life was that some innate power or some governing spirit was at the bottom of everything? Before animism, in the sense that all things had souls, was a current belief, the primitive mind seems to have regarded all nature as self-moving like themselves. For the notion of spirit is a late abstract notion. But when a power or a spirit, good or bad, had to be managed, it is perfectly obvious that water itself, that strange triple-natured liquid, should have become the subject of magic. Long ages before Thales, humanity had recognized that it was in many ways the basis of life. They attributed to it remarkable qualities, and when the Hebrews spoke of it as "living" water we should do the nature of language wrong if we considered the adjective was employed merely as a metaphor. To us it seems natural if we are by a river or a pool in hot weather to strip ourselves and plunge into it. But this is by no means the attitude of many savages even at the present day, and in the far-off beginning of time to do so obviously risked placing the bather at the mercy of the naturally untrustworthy fluid or, later, of the spirit which lived in and moved all water.

Among savages nothing answers to our conception of the natural. Disease is not natural. It, like death itself, is the work of an enemy. It is the result of the evil machinations of those who hate men or a man. But all evil is not wrought by spirits or magicians. Even now there are material agencies of a horrible kind. In Australia there are no dangerous wild beasts; but the fearful mind of man invented them. Terror is infectious; the aborigines have made many white converts. When I was working in the bush I was often entertained with vivid accounts of the Bunyip, that imaginary dreadful animal which, as I was told, is at least as big as a horse, and is often to be heard roaring at midnight in deep water-holes or rivers. Although I was then young, and had not any conception of anthropology or, indeed, of psychology as more than words, I was much struck by the fact that a large number of uneducated white men were easily led to believe in the existence of this creature. They were highly superstitious, and superstition is the imperfect functioning of ancient organic belief. If then even death is not natural to the mind of primitive man, and if he attributed self-acting malignancy to natural agents, it seems perfectly obvious that drowning was to him the result of a deliberate act on the part of evil water, and later, of that water's malignant spirit. No one will need to be reminded of the legend of the Lorelei, which is but a romantic survival of the early beliefs of man connected with streams and water. Even at the present day, in many of the rivers of Germany, to bathe at a particular time during St. John's Day at midsummer is an exceptionally rash and dangerous proceeding. These beliefs are found along the Necker and the Saale. St. John himself has really become a river god, or has taken the place of one and is, as Frazer tells us, especially greedy at Cologne, where he requires fourteen victims, seven of whom must be drowned in the river, and seven more who must break their necks by climbing. This second sacrifice shows that St. John has also replaced a tree spirit.

We should entirely misconceive the evolution of ancient thought if we considered all this was nothing but a result of the romantic imagination. It is hardly going too far to say that there is nothing romantic, however beautiful if may seem, which has not directly descended from the darkest superstition. In a short space it may be hard to convince the incredulous that bathing was wholly unnatural to primitive man, but they may, at least, admit that there is sufficient reason for suspecting that, however necessary water might be in the dawn of humanity, and perhaps because it was so necessary, it was looked upon as highly dangerous. How then did bathing and washing arise if this was the case? It is not straining logic to infer that both were the result of the very power of water which was feared, for to the untrained imagination the very things most to be dreaded, if managed by a skilful wizard, become the most efficacious aids to health or success in life. All members of the medical profession still represent the great magical element in the human mind as distinguished from the essentially religious, and may be said to take somewhat similar views as regards drugs as the early magician took with regard to the employment of dangerous natural agents or evil spirits. Those who prescribe arsenic, prussic acid, and many dangerous alkaloids, should certainly be able to understand the attitude of the early magician or medicine man who, having discovered the powers of a given spirit, or the vehicle in which it inhered, proceeded to employ it in definitely arranged doses of ritual. Among magicians there were also such differences of opinion and practice as are seen in modern medicine, for although bathing in many parts of Europe is forbidden, or regarded as daring on Midsummer Day, yet in certain places, especially in Sweden, to bathe on the night between Midsummer Eve and Midsummer Day is especially healthy and curative. So some physicians uphold Nauheim, others denounce it and all its pretensions. It is certainly held in Sweden that on this particular night water has extraordinary magical therapeutic qualities. In the old days such a midnight bath was especially supposed to strengthen the legs. It may, then, surely be taken for granted that washing was originally an unnatural and special process. It should not be difficult to show doctors that it is still as hard to convert the uneducated on this point as on that of ventilation, since, as students, they have had to attend their due number of outside midwifery cases. What then was the reason for washing, and how did bathing and swimming become a custom? We may say definitely that all contact with water, except that used for drinking, and perhaps even that, was definitely purificatory or medicinal or magical. Even the still surviving " grace before meat " is probably a protective incantation. But long before such ideas arose primitive man held that all natural agencies were infinitely suggestible. He hypnotized them with ritual, and they did what they were told to do if the rite was properly performed. Balneologists may therefore look upon themselves as recognized descendants of those ancient practitioners who employed powerful and dangerous waters in early magical therapeutics.

The history of evolution, as read in the scanty but pregnant documents of anthropology, is difficult to decipher. It resembles an organism which shows obscurely by rudimentary and dwindling mechanisms the processes of past growth. Yet some things are sure. In the million or two million years of the life of man the animistic and pre-animistic periods cannot be divided. Both theories survive still, and if animism is perishing, and the magician's view is crescent once more with the advance of science not falsely so-called, it will take immense eras of time before it becomes dominant. It is, therefore, not inconsistent with facts to mix "spirit" with water pure and simple, when these problems are dealt with. Some of the beliefs of existing, or lately existing, savage races show this confusion plainly, and the highest authorities do not always find it possible to distinguish between 1,000,000 or 2,000,000 B.C. Whatever the evidence was the truth would still be a matter of doubt, for even what happened yesterday may be a matter of conflict to-day. Yet we cannot scorn evidence which is written in the very nature of the human brain.

Sacred wells are found all over the world. It is obvious that they are now sacred because they are inhabited by powerful spirits, or presided over by conquering saints who displaced their predecessors; but it must be remembered that spirit is no more than a hypothesis to account for the powers and actions of any given thing, seeing that according to savage theory nothing can happen, as we say, "of itself." To such a degree has the belief in the magical efficacy and danger of water been impressed on mankind, that many people appear to have instincts concerning it or, if not instincts, certain semi-instinctive nervous affections which, without any particular reason or, so far as can be discovered, without any definite cause, become affections resembling phobias. If it is possible to discover by analysis in psycho-therapeutics the deeply hidden underlying cause of many nervous affections, it may be that some disciple of Freud might be able to prove to me that my own dread of deep or hidden running waters is not instinctive but curable by discovery. Nevertheless, at the age of four or five, I had a peculiar horror of wells. Even now I cannot approach a deep or dark one without mental disturbance. This is not due to the fear of depth or the mere possibility of losing my life by falling into it. When a seaman, I had no fear of going aloft, and when climbing in the Alps in later life I have been suspended by a rope over a precipice three thousand feet in depth without any sense of alarm. But even now, hidden running water affects me with fear, and I recollect that when, as a boy of twelve, I read a story in which murdered people were disposed of by being dropped through a trap-door in the floor of a house situated over a running stream I was deeply, and possibly permanently, affected. Such phobias, whether instinctive or the result of sunk and forgotten stimuli, are not infrequent. It seems possible that there is some instinct in man with regard to water, its dangers, its evil or beneficent effects, and that this instinct is against bathing, not for it.

Among savages, as we might reasonably expect, water cannot only achieve miracles, but is also liable to be affected by the conduct of men and, especially, the conduct of women. The savage ideas with regard to menstruation are familiar to all. Some remnants of it, as we know, still exist among civilized races; but in certain parts of the world a woman in that condition is obliged to purify herself in other ways than by bathing, for if she did bathe she would destroy the fish and dry up the river. By the stern reasoning of the uncultivated early mind contagious magic of this sort is carried to odd but, in its way, logical extremes. Such a woman in many places is forbidden to eat fish. This particular taboo is only found among races where fish is of importance. In most cases the use of water for purification seems to be imperative. If menstruation is dangerous and deadly, childbirth, in many cases, is still more so, and a miscarriage or a still-born child is something that requires more rites and more water than any other feminine phenomenon. Among some African tribes a concealed miscarriage seems to be more deadly than anything, and a woman who has procured abortion can kill a man by lying with him. The medicine man makes a great deal of this, as we can guess. Earth from the spot upon which she has buried the child has to be put into the river, while the place itself is sprinkled with water, and she has to wash for several days with water in which earth has been mingled. After that we shall all have rain again. From examples of this kind, and many others to be found in Frazer's *Golden Bough*, it may be inferred that much of the use of water is sympathetic magic to get rain, while it must be remembered that any unseemly and wrong act of man or woman may not only dry up the springs, but the very sky. There are, of course, many means of ensuring rain. Twins are especially powerful in this branch of medicine, and can readily be obtained by physicians. On ancient principles it might be argued that there would be little danger of drought if obstetricians, meteorologists, and balneologists worked in combination, although, in many cases among certain tribes twins are regarded as a highly dangerous and abnormal product: they are even killed in order to get rid of them.

Since water is so dangerous and powerful, washing of any kind often appears to be something of a ceremony. It is not therefore wonderful that washing the head, the most important part of the body, is a very serious matter among many races, even those called civilized. Among the poor a bath of any kind appears to be an ordeal, not a luxury. In Siam one observer knew a native preacher who washed his head monthly. The whole process took three days, one for preparation, one for the tremendous act, and the third for recovery. In old days the King of Persia had his head washed once a year. Roman ladies washed their heads every thirteenth of August upon Diana's day. It is probable that the rareness of such an act and such a choice of time show that there was magic in the ceremony. It was believed by the early Greeks that Juno bathed once a year for the especial purpose of restoring her virginity. Even if the legend is poetic, it must have been founded on some ancient belief. It is, perhaps, regrettable that, even among the poets, there appears to be no reference to Jove's reflections on the subject. Bathing is a common act with many tribes before hunting. The Kyaks of Burmah bathe by day and night for eight days before they hunt the panther, and naturally enough bathe afterwards to get rid of the panther's influence and avenging spirit. Bathing as a rain charm is even now found in many parts of Europe, especially in Russia. It is instructive to notice that in many cases the most effectual charm is to throw somebody into the water. This appears an obvious relic of the time when human beings were sacrificed to rivers, streams, and the sky which gives rain. In Armenia the charm consists in throwing the priest's wife into the water. In the Islands of Celebes in Melanesia a priest bathes in order to procure rain. It is a common thing to drench the lame, blind, and infirm, with water. This certainly brings rain and, if the wet and afflicted victim curses with great vigour, no doubt the water hears the better, and the charm is all the more effective. For, god or no god, it is highly intelligent, and not only intelligent, but both kindly and savage.

It appears sometimes possible to insult a spring or outrage a river and procure floods. In the Canary Islands the Guanche priestesses used to beat the sea with rods when there was a drought. This was very efficacious, for it rose up in waves which probably caused the winds which brought clouds and rain. If man's native capacity for putting the cart before the horse, which is still the chief stumbling-block to science, be considered, this should surprise no one. In Sumatra and other eastern islands, when rain is needed, crowds of women go into pools and splash each other. When we see a number of boys doing this in our own country they are no doubt likely to cause a great deal of rain, for the ritual is very powerful. All we have to do to bring rain is to treat water in the right way. That is the essence of magic, for the water knows all about it, and perhaps occasionally confides in a special medicine man what the real trick is which will compel him to increase his floods. However intelligent the water may be, careful study will make man its master. Bathing before marriage among the Greeks was a magic fertilizing ceremony, for water is necessary to the growth of all the fruits of the earth. At Troy, down to classical times, maidens about to marry bathed in the Scamander, familiar to us in the *Iliad*, and said to the river god, "Scamander, take my virginity." As Frazer points out, this sometimes led to young men bathing at the same time, and if there were any untoward results they were fathered on the river god. In this way demi-gods seems to have arisen easily enough, since a river or a stream is a very powerful deity, and, like most other gods, gets his best effects through his generative powers. There are signs of this in all religions, though the notion may be highly sublimated. It was common in many cases for women to be given to the river, or sacrificed in it, for if he could be afforded the opportunity of fertilizing them he would be all the more powerful in fertilizing the earth. It may be that there are still gods about some of those springs which are held to cure sterility and impotence, at Orezza it may be, and perhaps at Buxton, Wildbad, and Gastein. There some magical balneologists call confidently on the radium emanation which must have been a powerful water spirit even in earlier times. There may be a substratum of truth in the most extravagant magic or myth.

It was said above that, in some senses, nothing that we can do is really natural. If then bathing was originally unnatural and dangerous, especially to those who could not swim, and found water and water spirits deadly, it remains to be shown how superstition of all kinds led by devious paths to washing, to purification and fertilizing purposes, and hence by slow degrees to medicinal uses, and finally to purposes of cleanliness. Primitive man never washed to be clean, he washed to get rid of some influence and, in spite of his ignorance and his mistaken magical views, there is, in many of these savage customs, essential rightness, almost scientific accuracy. If the method of trial and error, handled intelligently, is the main source of most advances in knowledge, magic often hits the actual truth. In Mexico the Huichol Indians during a drought take water from a sacred spring and carry it a long distance to the sea. Water from the sea is carried and put into the spring. Now, why is this done? Is it possible that no one sees the reason at once? The water in its alien surroundings will obviously be uneasy and uncomfortable and rise in vapour. What can be more natural than that? The water tries to get home, but both clouds of expatriated vapour meet in the heavens, cause clouds, and fall as rain. Here actual facts are mixed with pure animism and magic, for sea and river renew each other in a perpetual cycle. It is difficult to get away from proper magic. With knowledge man can do anything. By killing a so-called heaven bird the Zulus make the very sky weep.

Of course many ceremonies for rain are properly religious, not magical. They appeal humbly to the ear of gods. This, however, is a late and a degenerate plan. It is much better to be a sturdy magician, and get the best of the many powers of water or of nature by manly personal efforts. But enough has been said to show that the savage mind, even of to-day, does not regard water merely as a useful liquid. The physicists say it is a mixture of hydrol, dihydrol, and trihydrol, and they assert, moreover, that trihydrol or ice must exist even in steam. This may be wonderful, but primitive man knew long ago that water was a very dangerous and wonderful fluid, capable of pulling the leg of any one who swam in it. He knew it could even talk and converse. Even now those of us who are not magicians can fish a running stream and hear it utter faint lost words, although they do not understand what it says, and cannot control it. With a real magician it has to behave, but as there were few thoroughly instructed magicians, even in the most ancient times, not many will insist that primitive man went for his morning dip as a matter of course. Water had to be watched and learnt. It was best for a bather to take a magician with him when he swam. For some people it is even now best to take one when bathing in the sea, or he may be sent for to try artificial respiration. It may be said that nothing whatever comes by nature. All assurance in the face of any dangerous phenomenon is acquired with danger and difficulty. Habit and customs arise by disaster and repair, for both are, in essence, construction.

It can now be perceived how all these mixed and mingled ceremonies for magic purification by water gradually crystallized into habits. Little by little results occurred which were not foreseen either by initiate or hierophant. People gradually get to like water. Among some races swimming seems on the way to become truly instinctive. Any great progress of man has almost always arisen through accident or as a side-effect. All doctors are or should be professors of sanitation; and cleanliness, however it arises, has good results. In parts of the Pacific sanitary science may be said to have arisen from the practice of malignant sorcerers burning *nahak* or food refuse belonging to some one in the community. By contagious magic anything with which a man has been in contact is part of himself; it can therefore be hurt or tortured, and the man himself will be ill. Certain malignants who understood this used to go about villages and pick up little bits of some discarded orange or banana skin. They were burnt with ceremonies, and those who were ill sent presents to the sorcerer to stop his enchantments. As a result people were careful to be clean. When I was in Apia I remember quite well that it seemed remarkably well kept. So sanitary science arrives blindfold. It was created by a desire to avoid the possible evil of magic, while bathing comes no doubt out of people's desire to use its good effects of purification. The loss of magic may be a disaster. It is for men of science to bring it back purified as by water.

If living water, with all its senses and powers of magic, and the later water gods, must be reckoned dangerous in the sea and deep rivers and pools, it is naturally enough regarded as holy and beneficent, though very delicate in its taste, in hot, dry countries. Rivers are obviously capable of fertilizing the whole country. By their fertilizing influences the feminine land produces fruit. If that is so, why should they not fertilize women? In the East many rivers are capable of actual procreation as I pointed out before. Women therefore who are barren take to bathing in order to obtain offspring. If there are sturdy guardians of the sacred water they may possibly help a little at times. The virtue of wells, which leads to washing and bathing as a cure for barrenness, is known even in England and Scotland. In Northumberland there is a sulphur spring which used to work wonders in this way, and may be effective still for all I know. It is highly probable that chalybeate springs, such as Orezza in Corsica, gained this reputation justly in the case of the anæmic. Since religion took part in the ritual of life the priesthood, the later clergy even, have done their best with pagan beliefs, and therefore many of those old efficacious pagan wells became the property of saints. Not only magic but religion also works wonders. When they act hand in hand something is bound to happen. In India sterility is, of course, caused by evil spirits. In some places if a surgeon were to cure sterility by special surgical methods, he might actually find himself deified locally, just as General Nicholson, who died so nobly at Delhi, was deified on the frontier. It is quite possible that a quasi-deification or apotheosis takes place nowadays with some popular doctors. In other forms the human mind works as it did of old.

The views dimly adumbrated suggest that all bathing, medicinal or purificatory, is due to the original fear of and belief in the living nature of water, and that the reputation of all old baths or bathing, now held to be curative, was originally due to magic, is strongly supported by certain facts I observed while in British Columbia. Although there are comparatively few Indians nowadays in the dry belt about Kamloops, those of the Thompson Indians who still exist retain many or most of the beliefs of their ancestors. I was acquainted with few of them, but while working some miles from Kamloops I discovered among the brush when I went fishing some edifices looking like teepees or little wigwams by the side of the stream. They were constructed of sticks running up to a point at the top, teepee shape, and were big enough to contain a man sitting in a crouching position. Just under him a hollow was scraped. On inquiry an Indian woman told me that they were Indian sweat-houses or, as they are otherwise named, *keekwillie* holes, usually contracted by whites into *kegly*, which by itself means "low." She told me that as far as she was aware they were used for medicinal purposes, and I have no doubt that in this she was correct but, as I have discovered since, and some of the evidence is to be found in *The Golden Bough,* they were not originally constructed for any such purpose. The way they were used was this. A man or woman got inside them in a crouching position. Water was poured into the hole above which the patient sat, a rug or buffalo robe was draped over the entrance, while the squaws outside heated stones in a fire, and when they were red-hot rolled them into the water. That a like bath in certain cases of arthritis, or so-called rheumatic affections, may do good is certain; but it is quite impossible to believe that such a mode of healing was discovered except through generations of trial and error undertaken for other purposes. The original reason of such a ceremony arose from the desire to free widows or widowers from the probable results of contact with death. They required purification, and for this purpose they sweated for hours, and then were plunged, or plunged themselves, into the neighbouring creek, after which they rubbed themselves with small branches of spruce which had been stuck into the ground close to the little teepee. There is no more striking instance of the way in which magic at purificatory ceremonies might easily have become measures of pure therapeutics. Such a complete series of vaso-motor and peripheral stimuli may well have helped to cure grief. No real anthropologist will fall into the modern error of believing grief "natural." It is due, as the fathers of man knew only too well, to the actual influences emanating from the dead, or their active spirits. In view of such facts I think it may legitimately be inferred that curative baths of every kind began by the practice of magic, and that all such processes were reinforced gradually, as magic gave way to religion, by religious purificatory methods. These same Thompson River Indians were accustomed if they touched the dead to bathe instantly, or as soon as possible afterwards. Where these *keekwillie* holes do not exist widows and widowers were still obliged to bathe. Bereaved persons, even in modern times, are also compelled to pass four times through a patch of wild rose bushes in order to rid themselves of the ghost. Not only this, but it is still customary among these people to cut branches of the thorny rose bushes and put them in their bed or blankets in order to prevent the spirit returning to its old night quarters.

When the light that is thrown upon the natural working of the human mind is considered, it seems that the study of anthropology might well be made part, even if a late part, of the training undergone by the student of medicine. The old-fashioned psychology founded on introspection, which depends for the imaginary validity of its conclusions on the understanding of words, though still taught, might be dispensed with to the advantage of all concerned. Words alone, as progress is made from one verbal statement to another, inevitably lead to wrong conclusions by the very logical processes that they imply. It was for this reason, this double use among men of science of their own terminology and the psychological use of words with all their possibilities of error, that led to experiments on conditioned reflexes. Such work tends to show that all intellectual labour is, in its essence and in actual method, a series of reflexes responding to the peculiar environment of the worker. The study of anthropology may have very far-reaching results on the knowledge not only of ancient practices, but also on the conception of the brain as a mechanism. In all the branches of magic touched on, the fact is seen that mentation acts with astounding regularity, by way of definite irresistible reflexes following upon certain definite stimuli. Time and time again, in far distant places between which there has been no possibility of communication, new but similar practices arise. This cannot be explained on any theory but that of the human brain reacting definitely on like stimuli. There is no distinction to be drawn between chemical reaction in one part of the world and the other, provided that the temperature, and perhaps the barometric pressure, are alike. But a study of anthropology must lead to the influence that the biochemical reactions of the brain, the complexities of which we have simplified unduly by calling them "the mind," are, if we take into consideration the infinitely greater complexities of cerebration, upon the same level of certainty as mere chemical reactions. If this is so, anthropology itself will be of the greatest assistance in understanding the why and wherefore of all human cerebral development. One science helps another, for it carries a lamp; but when these lamps help each other the light may indeed be great.

Frazer has himself pointed out with much force that many of the soundest customs of humanity have sprung out of magic. But if it is true that all human progress, like scientific progress, depends on hypothesis and trial and error, hit or miss, no one need be surprised to learn that in many cases practices have arisen from magic which were, or might be, deadly to the race which practised them. Although it may be said that the whole essence of immunity lies in the phrase, "a hair of the dog that bit you," there are ways of taking the hair which may be destructive. In a cholera epidemic in Egypt some forty years ago, or perhaps more, a peculiarly holy man died of that disease. It was obviously necessary to wash his sacred body. It was therefore taken to a neighbouring pool and duly cleansed by his ardent followers and admirers. It will not surprise even those who have hitherto taken no interest in magic if it is suggested that so holy a man by his contact with the water must have given it virtues of his own. This, at any rate, was obvious to his followers, for, procuring utensils of various kinds, they bottled a portion of this holy water and took it home and drank it. The results, from a medical point of view, were deadly. Perhaps from the religious standpoint they were held to be efficacious when his translated followers joined their leader in Paradise. It is possible to conceive that many tribes in the history of the world when they made a miss in their experiments did not recognize it, and by repeating it wiped themselves out. A fool is rewarded according to his folly, and wisdom is only the recognition of results.

Something was said above about the general views held on animism, or the savage theory which imagines all things whatsoever have their moving spirits. This is not a primitive belief, for the idea of spirit is an abstract notion. Before the evolving human brain was capable of such an abstraction, man no doubt held the view that all things like themselves were alive. So deeply rooted is animism in the human mind that its last faint remains can be seen in many men of scientific eminence who cannot rid themselves of the theory of vitalism. The savage vitalistic, or animistic, view was a simplifying hypothesis, and like all unverified hypotheses led to extraordinary results, not all of them without danger. But many were certainly sound. All tabooed and unclean foods are held by anthropologists to have been originally sacred. They were living gods. This is undoubtedly the case with the pig, and in those cases where its once sacred totem qualities have degenerated into dislike and a taboo, such a degeneration in hot countries may have been for the good of the race. It may even be said that it would have been better if some of the notions of the Eskimos had survived among other races. Among them it is forbidden to mingle different and various flesh foods in one full stomach. The gods and goddesses of the different animals would be offended by the alien contact. Such views are dietetically valuable. In the same way the taboos concerning mourners and the insistence on ritual washing are obviously wholesome and scientific, as every bacteriologist would admit.

The whole story of the gradual evolution of medical theory and practice from magic and religion is one of unsurpassed interest which might well engage the life and energies of any student. To such it would soon seem clear that magic in its best and worst senses still exists in medicine. It has not been got rid of by the decay and passage of the older theory of signatures. But, even if drugs are still exhibited on the merest grounds of tradition, the fine magical qualities of human influence and suggestion are every day better recognized, and therefore no physician need look with contempt on his spiritual ancestors or even on his savage colleagues in far-off countries. It would be as wrong to do so as to scorn Hippocrates, Aristotle, or Galen because they did not know what are commonplaces to a first year's student. There is no new method of discovery and no real increase in the powers of logic. Those who seek truth are no more than an army marching in the dark led by the dimmest sense of orientation. When that is reached which seems an insuperable obstacle their battalions hurl themselves against it, and if one man finds a weak spot and overcomes the difficulty he becomes a leader and is presently called a genius. Such geniuses whose names and graves were forgotten a million years ago helped to bring man through great darkness, but not to any resting-place. It should be consolatory to every worker to remember that, even if he has not the great and happy fortune to light a new lamp in the world, his very errors and failures assist his fellows and all mankind to avoid like disasters in the time to come.

CHAPTER IX

The Physiology of Consciousness

IT may seem desirable to develop shortly what was said in the last chapter on the subject of psychology. It is a common and useful trick of the theologian to assert that the physico-chemical view of "mental" action is rapidly decaying. Such writers greet with enthusiasm any popular and ignorant reaction, even though a similar movement, if it militated against the loose hypothetical explanations they favour, would be greeted with contempt. Instead of yielding ground to the religious philosopher, those who advocate so-called "materialism" are daily taking positions from the introspectionists, and nothing but ignorance of physiological advance permits them to believe otherwise. The warfare in the body as construction proceeds has its true analogues in the modification of theory. It is true that certain leaders of thought have been carried away by their instincts, but this is due to the fact that many men of the highest eminence are only partially educated. To think on the lines of one science alone is to remain at the mercy of uncorrected traditional ideas in many departments of thought. Such lack real mental immunity. It therefore follows that not every man of science has the scientific mind which takes for granted the possibility of arranging all phenomena whatsoever in ultimate order. With the region beyond phenomena such a mind has no real concern: noumena may be left as a playground for those who like to waste their energy in those arrangements of words which are dignified by the devotees of theological and metaphysical jigsaw puzzles with the high-sounding name of the "Philosophy of the Absolute." So far as the problems of space and time are concerned they may be dealt with by mathematicians, and what is said of them by philosophers, with no knowledge of science, can be safely ignored. Yet, owing to early influences, even men highly endowed with the scientific spirit are apt in their haste to give away to the enemy positions which afterwards have to be recaptured at great cost. This has certainly been the case with "the mystery of consciousness." The many hundreds of years partially wasted in the verbal gymnastics of the schoolmen and their modern congeners and descendants have naturally left their mark. That they were not wholly a waste may be admitted, since reasoning accurately even on empty major premisses is a great mental exercise; but so far as the conclusions drawn became more than mere logical dividends their effect has been harmful. To free the mind from early impressions is never wholly possible, and the assumptions of the nursery may partially determine the mental action of the wisest, just as ancient instincts in a race produce effects of which the cause may be totally unknown. As a result it not unfrequently happens that consciousness is admitted to be an ultimate mystery, although every reaction of the brain points clearly to the fact that it is but a definite, though highly delicate, response to the internal and external environment. Huxley himself, being then, no doubt, under the influence of theories of mental and physical parallelism, incautiously admitted that the problems connected with it were impossible, or unlikely, to be solved. A very slight study of the history of science reveals, however, that the problems which are incapable of solution frequently receive it before the ink of the incredulous is dry, or, at least, before it fades.

There is no need to go into the work done on cerebral organization and construction. The names of Hunter, Willis, Horsley, Hughlings Jackson, Gaskell, Head, and Ferrier, to speak of but few, are sufficient witnesses to the labour bestowed upon the brain. With regard, however, to the special phenomena lumped together by the use of the word " consciousness," it may, perhaps, be admitted that Pavlov did, at the very least, just as useful work. He reduced such obscurities as " states of consciousness " into multiplex, or conditioned, reflexes, and showed that the nomenclature of most psychologists was at once otiose and misleading. It is too seldom observed that the mysteries of " mind " are no more than the result of ignoring physiology and the almost ineradicable instinct of man to consider that a word represents a simple thing. As soon, however, as " mental states " are resolved into reflexes among some of the 10,000,000,000 cortical neurons it becomes obvious that the word " mind " is no more than shorthand for neuronal action and interaction when influenced from the outside or by internal stimuli. There is no such thing known in " consciousness " as the brain acting as a whole. The cells may be, and probably are always, in a state of tone, for they would otherwise degenerate; but very few of them can produce motor reactions, of any kind, at the same time. Those reflexes result in action, even the action of " thought," which are stimulated to discharge, or at the least, excited to a state of tone almost sufficient to result in discharge. Tonus being a state of readiness for activity, all nerves stimulated by conduction from the discharging neurons may raise verbal centres to a condition near to a like discharge. In such cases we have verbal thinking, *i.e.* impulse not discharged over motor tracts leading to speech. These relations of raised or lowered tone, of inhibition or excitation, are obviously neuronal functions, and all "thought" is the impulse towards discharge in reactions, forced, useful, or pleasant, under definite stimuli exciting complex reflex arcs.

Such views, it seems, are easily grasped when we deal with the lower animals, but many find it difficult to believe that the poet's "consciousness" when he writes a poem is in fact a reaction to his internal and external environment, and that the poem is truly as much a reaction product as the bark of a dog or the spring of a tiger. There is, however, no real gap discoverable between the reflex responses of an amœba, whose irritability as protoplasm is of the same order, though less specialized, as that of a neuron, and all the spinal and cerebral reflexes of a genius. Such reflexes are, however, more and more complex and "conditioned," *i.e.* dependent on other reflexes and much more easily inhibited. In such a case inhibition probably means no more than a failure of some synapse to act, while excitation which results in such original graphic verbal reactions as a poem is the functioning of new nerve dendrons hitherto not joined up, and fresh combinations of older ones which have functioned before.

Certainly the case for such conclusions has been of late immeasurably strengthened by Pavlov. This physiologist was led to make his experiments by finding that the moment even physiologists touched "mental" phenomena they adopted another language than that used in their own work. He found by experiments on the salivary glands that reflex excitations could be made to depend on linked reflex excitations—that is, by reflexes conditioned by other reflexes. A dog's glands can be educated to act not only by the presentation of food but on the excitation of them by the sound of a bell. A bell of a few more or less vibrations fails to produce salivary action. A time factor can be introduced and the glands made to act five minutes, say, after the bell is struck. At each introduction of a new element into the linked reflexes the process is more and more "conditioned," and more and more easily interrupted by some accidental or purposed stimulation. This complex of reflexes becomes at last "intelligence."

It is commonly said that reflexes are nervous units. It is, however, sounder to regard the real nervous, or cerebral, or "mental" unit as the native irritability of the cell. If this is so the rise from reaction in the cell to a simple reflex, and from that to reflexes conditioned by others, and further to the most complex set of reflexes imaginable in the highest brain, should show no break. That we are unable to foretell the reaction in the cases of high reflex combinations goes for nothing. It is, indeed, our incapacity to do so which shows the nature of words. They are sound signals which produce or tend to produce reactions, thus becoming, on this analysis, links in reflex reactions. Their motor products depend entirely on the nature and quality of the organism concerned. Thus to mention the word "faery" in a mixed gathering may produce a "fairy" story from one and induce another to quote "perilous seas and faery lands forlorn."

If then such compound conditioned reflexes are the cortical apparatus for keeping in touch with the environment, with all its excitations and inhibitions, presented to it at the moment, it is reflexly forced upon us to declare that " consciousness " is the massed sensations of the thinker, or such a complex of them as may be most strongly stimulated. Since " memory " is nothing but the establishment of nervous tracts, and the act of memory a stimulation passing over a particular synapse formerly opened up, we can understand how " self-awareness," which is really " memory," consists of a set of opened tracts which stimulate other tracts, possibly motor ones, which finally may pass into reflexly induced speech or writing. " Self-awareness " thus sinks away from us on acting. That pointed and consecutive speech, dealing with the situation, may occur reflexly, is obvious to those who have seen operations performed under light anæsthesia in which the reflexes are not abolished. The patient may feel pain and abuse the surgeon in the vilest language. The good public speaker is one who forgets himself, ceases to be inhibited by fears as to his success, speaks over short paths rather than long ones, and loses " self-awareness " in semi-automatic or reflex emotional or logical utterance. What he says is rapid adaptation to his environment.

The difficulty experienced, even by some men of scientific training, in accepting such views of " consciousness " as are suggested above, is undoubtedly a reflex cerebral state, induced in early life by the stimulation or inhibition of words or repeated sound signals which have established regularly working reflexes. They have been taught to respond to these in a certain way, *i.e.* their education has opened up tracts of nervous discharge which prevent further analysis by inhibiting the opening of fresh neuronal paths. This is a phenomenon known to psychiatrists as "resistance." But though "resistance" to fresh stimulation taught in the shape of combinations of word signs or sound symbols is frequently accompanied with dislike of the "idea," by which we must understand a new set of reactions, it may be without any such dislike, and may represent only a temporary incapacity, under the weak stimulation of an inadequate verbal presentment of convincing analysis, to establish new nervous connections. The difficulties of dealing satisfactorily to all with such a subject, and the right way to attempt it, may be suggested by considering that the very word "convincing" just used is obviously shorthand or a symbol for the reflex opening of fresh neuronal paths which offer great synaptic resistance. Such views explain the physiological reasons that it is so difficult to convince the old. In them synaptic resistance tends to become synaptic block. The opposed phenomenon is observed in fixed ideas, and in mania, where over certain tracts there are what may be called "fused" synapses in which the gemmules for pathological reasons do not retract until exhaustion occurs. I suggest, then, that when a man like Huxley, a very powerful stimulator, asserts consciousness to be a mystery, such an assertion is likely to inhibit speculation on the part of others, such inhibition taking the form of saying, "if a brain like Huxley's found it so, is it likely that I should ever get to understand it?" It must, however, be remembered that the whole history of science might be mapped out in a series of statements as too "impenetrable mysteries" which have proved themselves capable of easy solution. I remember being much struck by the objection of an otherwise capable man of science to a view of my own, which was afterwards proved by experiment to be correct, on the ground that if it were true it would have been found out before.

The resistance or dislike to the analysis of consciousness into combined conditioned reflexes seems particularly strong where it deals with the emotions. To analyse a religious attitude into reflex correspondence with an imaginary or constructed environment, such construction being in fact the co-ordination of rigid nervous tracts, is regarded as "materialism," or a gross incapacity for taking "spiritual" views. Such opinions, however, are not worth combating, as they are usually held by those without physiological knowledge. But those who merely regard consciousness as a mystery, probably not capable of solution, often find similar difficulties. They may say, for instance, that though emotion and volition have their concomitants in molecular changes in brain matter, no material qualities, such as weight and occupancy of space, can be predicated of them. An emotion, however, is only a "mental" entity till it is discovered to be nervous discharges over certain short circuits in the brain through which the motor impulses of instincts have passed during long stages of evolution, all such discharges being accompanied by vaso-motor phenomena.

If this is so, and no physiologist will deny it, space and position, vascular dilation and contraction, and the possible measurement of nervous discharges across resisting synapses can actually be predicated of the highest emotions. An emotion is thus no entity, it is not a thing properly to be described in a word, though it may be designated by such a symbol and act as such in a reflex chain of suggestion: it is, in fact a very complex bodily and cerebral state, easily distinguished from a purely "intellectual" state, which is a cortical process not going on over ancient instinctive paths, but over the pyramidal tract, through the cells of Betz, without as a rule any vaso-motor disturbances. Such disturbances, however, often follow upon intellectual discovery as the results of attainment and, to speak in terms of energetics, of energy suddenly freed. Kepler's emotion on being "freed" by his great discovery is a good example.

However little such an analysis may commend itself to the more ancient psychologist who lives in a world of words, it is certain that it is only upon such lines that scientific explanation can proceed. It enables the physiologist to do work without being confused by the necessity of defining terms relative to consciousness about which no two philosophers are at one. So far as science is concerned it may be taken for granted that cerebral response to the internal and external environment, acting reflexly to excitation and inhibition, is not correlated with consciousness, but *is* actually consciousness itself, including the subconscious and unconscious: the subconscious being tracts in nervous tone which may easily discharge themselves in motor reactions at any time if normally stimulated, while the unconscious consists of other tracts only resulting directly in motor reactions under abnormal excitation or pathological conditions.

CHAPTER X

The Psychology of Training and Organization [1]

IT may be that some of you were alarmed by the word "Psychology" appearing in the title of a lecture which you had orders to attend. Possibly it opened up to you the prospect of illimitable boredom. I own that it is a subject which, with very little care, can be made both boring and obscure. Many writers when dealing with the mind obtain the two results with ease. But straightforward psychology is not metaphysical word-juggling, and I hope to make what I have to say as clear as orders should be made by those who issue them. Psychology is nothing more than the way our minds work, and I should like you to remember that the word "mind" is just useful shorthand for the working of the brain. All of us have some notion of what affects us or leaves us cold. We respond to stimulation, we act or refuse to act. You know what you like and what you dislike. Perhaps you even know why you are here at all. Certainly you would not have been at a lecturer's mercy if you had not been moved by your minds, your brains, towards common national ends. Many different reasons may have in-

[1] Lecture delivered at Purfleet Camp to the members of the O.T.C. (Capt. B. C. Lake, O.C.) and the officers of the 7th Reserve Brigade. 1915. Although it is not an integral portion of this book I have given it a place for reasons which will possibly be obvious to those interested alike in science and in psychology, now rapidly becoming a science.

fluenced you, but on the whole I take it that what moved you most was a sense of duty combined with a desire for the splendid natural activity of a military life. Yet behind all your feelings there was something else, something bigger and something which, though really obscure, is not beyond comprehension. Most of us in life do things, and believe we can say why. We use our intellect to make apologies for our own actions, and sometimes succeed in the task to our own satisfaction. And still we may wonder in our hearts whether there was not some instinct in us that was the real motive power. Again, many of you must have felt the heavy weight of our economic civilization, and to become a soldier is, in a way, to get back to nature. You therefore come here to be trained, and to learn to train others, in the very ancient organization called an army. Busy as you may be, you should be free from many of the worries besetting those who are all "on their own." Discipline and control may obstruct some activities, but they leave others free. Young men especially like change. Here you certainly get it, sometimes to your surprise. Besides these reasons for your actions there is the other reason which I hope presently to make plain. As a hint, it may be said that, though in some ways you are now more yourselves than you ever seemed, in another and a very strange and not unpleasing way you are less. You will exercise powers you never had yet, and will be restrained in ways you would once have resented fiercely.

Let me phrase it plainly. You are here as grist for the military mill. You have to go through the machine. The reason of many of the processes through which you are put are probably obscure to you. Some seem a little absurd, some too severe, some, perhaps, totally unintelligible. You wonder why you are being trained in such a way, and why it takes so long. Yet, though you may have found many of your experiences exasperating, the experience of others has shown them to be just what is needed. Your very exasperation is part of your course. You have to control it. Being put under arrest has helped to make many things clear to those who can learn. Not all of your superiors know the deep mental, or cerebral, side of the processes of training and organization but, since an army is a continuing live organism, they have tradition, military history, and their own experience in the making of a company or battalion, which show them that certain results follow on the adoption of particular methods. We all use words and phrases of which the real meaning may be unknown to us. We often employ the French phrase, and speak of *esprit de corps* as the end and aim of training. The "spirit," as we say, makes the body live and makes it one. This is shorthand, but it is true. Every soldier knows it, but not every one could tell us why, even if he has *p.s.c.* after his name. If our methods are right our reasons may not matter. But as no methods are perfect, even when moulded by age-long tradition, knowledge of underlying causes may help to improve them.

I spoke of an army as an organism. It exists as a body, it has members, tools, a brain, a nervous system, and all are used to ensure that certain effects are produced. An army, too, can suffer and rejoice. It can become irresistible by continued success; it can suffer panic, and it can die. These words are not mere illustrations. You and your officers and men are living parts of a living thing, even though the staff may never trouble to look upon an army in that light. They may not be so self-conscious. Perhaps that is all the better for them. It is best not to think of ourselves. Note well then when a part of an army begins to think only of itself, and by itself, there is danger of disaster, perhaps of dissolution. A healthy man never thinks about himself as parts. Only sick people do that. When all things work together easily that is health. Your officers know this is true of a company or a battalion, and of themselves. If the nervous system is out of hand, the whole body goes to pieces. They are the nervous system of the part they command, just as the Headquarters Staff is of the whole. It is well to know this, but not to brood on it. Knowledge should sink in and become wisdom—a proved instinct. If the Staff knows this practically it will work all the better, with greater certainty. If there is friction and separation at Headquarters the whole body suffers.

Now, although the training and organization of an army make a special branch of study, all organized bodies can be analysed by similar methods. At first it may seem difficult for you to understand that there is a real resemblance, a true analogy, between the workings of a committee or corporation of any kind and an army. Nevertheless it is true that an army is something very different from the individuals who compose it, just as a committee or any corporate body is different from its members. An organized body with a head and subordinates will do things which none of its members would or could attempt. Motives affect it differently. The whole is another thing than the units. If the motives and stimuli affecting it are of a high and noble order this organized body will move instinctively towards great things that might not have moved its members singly. Their better instincts are appealed to. They would be ashamed to show they think of themselves. In such an atmosphere a mean man may become generous. When he has gone away he may be himself again, and contemplate the counterfoil in his cheque-book with rueful astonishment. If the motives moving such a body are not high but, let us say, purely financial, it may, on the other hand, do things which come to be regarded by the very men who voted for them as utterly detestable from their individual point of view. Conceivably such a corporation might discharge an old servant without pension, and the very man who moved the resolution might possibly support him afterwards. For such reasons, however shortly and roughly presented to you, we may infer that any organized body is a real organism because it acts differently from its units, and has different motives and different ends. Purely individual training is useless for bringing out the qualities and powers for which such a body has been created.

Such considerations as these have a great application to your physical training. Many of you thought you were well and strong when you came here. Perhaps you know now that you were neither. You have found out what health is. It is being all one, it is forgetting you have parts, since all things in you work together. A breakdown in your health might mean a breakdown months hence in the moral health of a platoon, and a disaster. Your physical well-being is essential not only to yourselves. There are glands in your body which give you courage in emergencies. If the adrenal glands fail you might be cowards. When training reaches a high pitch and you feel the strain of it you will remember that a greater strain must come on you later. That is why an army is trained severely and the incapable are weeded out. A breakdown of one may mean the breakdown of many. In the face of difficulty we need to be well and strong, and cheerful companions. Thus we give out encouragement as a gland may yield powerful stimulation in danger.

You will see then that the beginnings of organization exist in all of us: in the whole human race. We are gregarious, unless we are ill; we commonly associate together for ends that appeal to us; we form clubs and societies. It is in our nature to do so. Without desire of common action there could be no social life and no progress. Altruism, or thought for others, exists in us all, though perhaps only as a seed: we are always prepared to make some kind of sacrifice for common ends. We cannot live alone.

Many of you here have been Public School boys. English schoolmasters maintain that the chief end of a school is not direct preparation for actual life but the production of character. How far they succeed I should not like to say in this place, but so far as they do it is because the boys are trained to work in teams and taught to sacrifice their ease and leisure, and even their hopes of distinction, to the honour and glory of their school. So the essential thing is not mere production of what is commonly called an upright character. Such a person may be incapable of working with others. What is wanted is the production of a character as a fit part of an organization which can subdue all self-regarding instincts and impulses. If a young cricketer is a sound member of his school, being turned down for the First Eleven may, indeed, be bitter, but if he recognizes it as just he takes it, as we say, like a man. For to be "like a man" is a great thing. It implies endurance, courage, self-restraint. Such a boy learns to trust in the judgment of others who have proved themselves, and he knows that loud revolt is not playing the game. Submission of this order is necessary to all of us, but most of all to soldiers. We see that in a good school there are all the rough essentials of organization and training. It is a corporate body, and the boys when at school are different from what they are at home. They think differently and feel differently, and that means they *are* different. Thus parents and schoolmasters may have very different opinions of the same boy, and what is more, the parent may be right if his son chooses some more solitary profession, while the master may be right if the boy goes into the army or navy. Many of you, no doubt, remember the sense of loyalty to your own school which grew up in spite of the brutality of a few of your fellows, and also in spite of the peculiar hostility which you felt towards some, if not all, the masters. And yet, if they were at all decent, you would have been ready to maintain they were better " beasts " than those of any other school. This feeling of semi-hostility between the trainer and the trained is, perhaps, essential for good results. It implies resisting stuff in those who are being moulded and organized. With good tools you can forge anything out of steel, but not much can be made of putty.

Possibly you now begin to recognize some strange resemblance between your own feelings as officers in training with those you had as a schoolboy. Day by day you are learning to suppress that part of yourself which for ever contends you have the right to do exactly as you please. You do not go about insisting on your rights. It is not good form to do so, and that means it does not really pay any one to be selfish. Perhaps, too, you have begun to see why you must be young to be trained. The very old are mostly set and rigid, and cannot easily rid themselves of ancient ideas. At any school a boy who has not been trained to obedience at home has a hard time before he learns that somebody a year his senior is not only likely to tell him to do a thing but to see that it gets done. In training for the army there is, of course, a great advance of thought over a school, for you come here duly prepared to surrender your personalities or part of them. But even so it is in many cases a very difficult process. You recognize that it is necessary, but perhaps you do not see the mental side of things which makes it necessary. Yet to know why makes all things easy. I dare say you have already compared in your minds the curious semi-hostility there often is between the combatant branches and the Staff of an army with that between schoolmasters and their pupils of which I spoke just now. The masters initiate and carry on a process of limiting natural freedom. The resulting hostility, or armed neutrality, is, in its way, a good thing. It inspires action and emulation. When a thing is inevitable, if it can be turned to good so much the better. Obstacles balking fools the wise make pivots of victory. Since the Staff of the army is the brain of the army it is obviously different in its functions from those who do the active work. When an officer with red tabs on his uniform and a red band round his cap comes to the trenches, looks at them, makes a few casual remarks, goes away again, and you are presently told that everything done has been done wrong and has to be done again, there is, of course, a kind of revolt against it. Sometimes you may say bitterly that the Staff has not got to do the work. But possibly you may recognize as a compensation that it could not do your work if it tried. I am sure that it could not after it has been trained on its own special lines for any length of time. But you must remember that this partial incapacity is a sacrifice to efficiency in the organization to which you all belong. The Staff make their sacrifices in the same way as the combatant branches make theirs. They give up a portion of themselves for the benefit of the army. It is a curious fact, with which many of you are probably not acquainted, that, though most of the cells of the body are capable of reproducing themselves and do constantly reproduce themselves throughout the life of man, the nerve cells are incapable of reproduction. The nerve cells and the whole brain composed of them, with which you begin your life, you carry to the grave. This may be a heavy burden to some unlucky people, but if you think a minute it emphasizes amazingly the necessity of training the nerve cells of the brain and whole nervous system at an early stage of their career, and it shows us why we should endeavour to keep them elastic. The man who keeps his mind fresh through life is the man who is interested in everything and is never stale or satisfied, for he gives his nerve cells all kinds of work and keeps them in training. But if he belongs to a highly specialized branch, such as the Staff of an army, he must necessarily sacrifice, or partially atrophy, much of his capabilities and capacities for the definite ends of the body to which he belongs. If the chief nerve cells and the main nervous system of the army do this they must be very different from those of you who are to be, if I may put it so, the nerve endings in the muscular portions of the army organization. Those on the Staff have much more to take into account, and above all things must avoid being affected by the corporate enthusiasm which enables a battalion or a company or even a platoon to be carried away by the passion of the fight. If, therefore, the Staff keeps itself aloof from these passions, as indeed it must, it often seems cold and alien from those who do the actual work. For efficiency all must sacrifice something of their individuality. All their nervous power must be turned towards the common ends for which they have been trained. Our individuality depends largely on the points in which we are, or believe ourselves to be, different from other men. But when working together for common objects we cannot insist upon our differences. If we do insist upon them nothing can be done. I have seen this happen dozens of times in badly organized committees. Agreement on common ends, touched with mass emotion and mass feeling, is the true basis of organization.

I do not suppose that many of you have made a study of what is called the psychology of crowds. Many years ago I began to work on the subject, and to my great annoyance a very able Frenchman, Gustave Le Bon, published a book called *Psychologie des Foules.* My only consolation is that he probably did it better than I should have done. It is brilliant and suggestive. If you pursue studies of this order you will find that there is such a thing as natural partial organization of individuals without preparation and without a real nervous system. When in crowds, you have sometimes found yourselves carried away without knowing the reason. You have perhaps shouted in a manner totally at variance with your common habits, you may have been ready to assault people or to break the law in the most enthusiastic way. I remember many years ago, when I was ill and thought exercise would do me good, going out for a long walk on one of the days of the Epsom Spring Meeting. It will perhaps be hard for me to convince you that I came out on Epsom Downs without knowing where I was. When I found out and saw the big crowd in the distance I walked towards it. I was gloomy and dyspeptic. I never cared much for racing; I had never attended a big race meeting in my life. But I said to myself that as I was there I might as well see what they were doing. Bit by bit I edged my way into the crowd and presently forgot I was ill, and began to take an interest in things. I asked the man next to me what race was to be run and, obviously showing great surprise at my ignorance, he answered that it was the Oaks. And he told me that the name of the favourite was Geheimniss. Presently the race was started, and soon the crowd rose up with a roar as the horses came round Tattenham Corner into the straight. Now do not forget that I had not a penny on the result, and had never seen a race before, and did not know the name of a single mare in it until I was told. Yet when they came up the straight, and the crowd began to shout, and the whole of the people in the Grand Stand opposite rose to their feet, to my utter amazement I found myself shouting at the top of my voice, " Geheimniss wins, Geheimniss wins ! " Somebody near me, on the other hand, having probably put money on another animal, shouted, " So-and-so wins, So-and-so wins ! " whereupon I turned upon him furiously and said, " No, damn you, Geheimniss wins ! " And she did win. Then the crowd broke up and I drifted out of it and went off by myself wondering what had happened to me. I know now that I had been caught by the massed enthusiasm of the crowd and made one of a very peculiar racing organism—an organism, by the way, not of a high type.

Here I think I might quote a short passage out of something I wrote many years ago :

" A crowd is not human, as we understand human individuality. It's not bestial, not reptilian. But it's all three—human, bestial, and reptilian . . . . A crowd is a flood of life, a giant mass of deadly forces ; it has no very clear foresight ; it takes the present only ; it has no conscience. A man in a crowd can commit crimes and come out without knowing he has done so. It's a new organism, a creature not in the books. Drag one of its parts out, knock him down and cool him, and see him come back slowly to humanity again. I've seen it. The psychology of a crowd is the psychology of a pure instinctive. A madman may act as a crowd acts; he's gone down to the raging level of a mass. The hot average of an angry number is a devilish thing."

Some day those of you who remain in the army may be called upon to do the most disagreeable duty of a soldier, which is to act in civil disorder. Then you may unluckily learn the nature of a savage crowd. Instinctively you will recognize that it is an organism, and see that it has a strength of passion single in aim. You will endeavour to split it up, to keep it moving ceaselessly lest it should get set and act. We may be thankful to know that these very qualities and passions directed and trained and organized can give us the army which we need.

In the crowd considered as a simple organism a single thought almost always dominates. It cannot, as a rule, be a lofty or ennobling thought, for without much training a mass of people are rarely capable of being moved by fine motives. Therefore you need not be surprised when you recognize the fact that an organized crowd in action is often a destructive organism. You may see the same in strikes where the nervous system of a trades union fails to control a badly organized body of men. Of course in every crowd there are the rudiments of a nervous system: somebody springs to the front and becomes a ringleader. If he speaks, and speaks successfully, he always represents what the others want and usually puts it in a few words. What we are accustomed to call contemptuously an agitator is nothing more nor less than a possible part of the inchoate nervous system of a new body. By the time it is properly organized we usually cease to call the leaders abusive names, and if they keep a firm nervous control over their numbers we are prepared to do them more or less honour.

I think we may now say that, when we have specific training for specific and definite purposes, we must have a union of organized ideas, a common end, and a nervous system. I dare say you have often been bored to tears, almost to extinction, by some of the training through which you have been put. This I know to be especially true of the men who are, or will be, under your command, when they are going through the long early stages of their drill. Although on general principles it is not essential to explain definitely to most recruits the whole purpose of their training, I think the time comes when the men should be given some of the real reasons why they have to do things which weary them. Sometimes you will hear a private declare he wishes he had not joined the army, because he has been " forming fours " for twelve months at a time. He cannot understand why he and the battalion to which he belongs are not yet considered ready for the Front. He will add that when he gets there he does not suppose he will be kept " forming fours," and he cannot understand why he should learn it anyhow. He does not know that till it bores him in some measure it has not become instinctive. And it has to be instinctive, because only so can his nervous system learn to respond rapidly to orders. Perhaps it is in some ways the same with you. Possibly you wondered why you were put through so much drill or why you had less than recruits. It was not because you were more intelligent, but because as the last ends of the battalion's nervous system you had to preserve more individuality than your men. Possibly in certain cases you might explain to them why you are drilling them—why you want so much to get them to act together, to act with rapidity, to act instinctively or even like a reflex. And a reflex action, I may remind you, is one that has a short path. It is not pondered over by the intellect ; it is done more than instinctively, it is entirely a decentralized action for which no reference need be made to the higher brain. Rapid reflex action to the stimulus of an order are the essential results of all good training. When there is no time for reasoning, when action is absolutely necessary, nobody must stop to think. But in a properly trained and organized body of men, whether it be large or small, the thinking has been done, the men have learnt to obey ; while the officers have learnt not only to obey but to understand what is wanted almost before the order reaches them. They are adapted to their new environment. You have, of course, already been instructed that when you get into the firing line, and are in command of a platoon or a company, and the order is given to advance, it is your place to lead. Probably it is true that the best leaders are those who, on such occasions, are out of the trenches themselves before they give the order. Now if this is so, it is what we should expect from the very nature of the organism I have been trying to describe. As you see, the platoon leader or the company leader is essentially the nervous portion of the platoon or company. But, as you must know, when a nervous impulse comes from the brain it is obvious the nerve is in action before the muscles react. So when you get the order, or the time comes for you to move, you necessarily do so before your men. Where you go they will follow if they have been properly trained and are in good condition. I may suggest to you here that the phrase " shaken troops " means, in very many cases, that the nervous system of that particular portion of the organism has itself been shaken and broken up. So long as the nervous system of the body keeps its power of reaction to a stimulus, so long will the muscles move. If the nerve fibre which moves a muscle is disintegrated or gets paralysed the muscle will not stir. If a nerve end is wholly tired out the impulse is not carried over until it is repaired by rest. We know, however, by physical experiments that, after a muscle thus ceases to contract, it will work again if stimulated directly by an electric battery. From this we see that it was the end of a nerve which temporarily broke down, and that the muscle cells still keep their contractility. You may therefore learn from this the high responsibility that rests on you to keep absolutely fit in mind and body, for the fitter you are the longer you will be able to transmit energy and carry out the orders you receive. In most cases you need not doubt that if you have men who will not move when you order them, the fault lies either with you as a leader or with the training of the men before or since you took them over. But be readier to blame yourselves than others. Of course you will see, on the theories I have just suggested to you (and remember I have no time to do more than make suggestions), that much depends on the quality of the nerves and the quality of the reacting muscles, on the quality of the officers and the quality of the troops. It sometimes happens in war, as you must know, that the men who will not move for one officer will for another, and that on desperate occasions the Commanding Officer himself may have to come into the firing line and take the place of the Company Officer. This may remind you of what I have just said of a muscle being directly stimulated by electricity. I can give you a curious example of this which happened at sea in a merchant ship. Organization in such a vessel is always rough and ready, but still it is organization. In very bad weather, when going aloft is highly dangerous, the men sometimes will not move for the second mate, whose duty it is to lead them when they are ordered aloft. In this particular case they refused to follow him. It was blowing a hurricane, and it was necessary to cut away a topsail rather than stow it, because to have started a sheet would probably have brought the topmast about their ears. When they refused to follow the second mate the mate attempted to lead them. One or two made a motion to follow him, and were actually dragged back by the others. Then the old skipper, a white-haired man of over sixty, came down from the poop, and without a word climbed on the rail. Just as he was about to put his foot on the ratlines the men with one accord rushed after him, pulled him down, and went aloft themselves. You see they succumbed to the greater stimulus when they had failed to move for the less.

Perhaps I may venture here to say something to you about the men who will be under your command. They will become much to you and you will be much to them if you have in you the power of command, which is the nervous force of organization. There are many ways of doing things. With men the means are love and fear. Your men should love you, and they will probably express it by saying, "Oh, the officer's all right!" You can, of course, if you have the power in you, make them fear you. That is one way of working, but if you make them love you they will fear you too. We fear those most we love most. If any of you who are bachelors doubt this, ask those among you who are married. Well, there is something to add to this. Many a local success has been gained in war by the rage of men at the loss of a beloved leader. But I have never heard of one gained because a man they feared and hated went down in battle. Remembering that all of you make up an organization, you will each in your own way reach out to your men and grapple them to you. In chemistry an atom is said to have much valency or little or none, and its valency is its power of hooking on to other atoms and making a big organized molecule. Might I say to you, get as much valency as you can? You may develop it by learning to help, by sympathy, by understanding. There are chemical compounds called paraffins, and the word etymologically means "little affinity." Paraffins are compounds of satisfied valency: perhaps we might say they are fatly self-satisfied. They cannot hook on to anything. Do not be paraffin officers: take your men into your grasp. You will get some of their best qualities, and you will give them some of yours. In a very deep way you will become like them and they like you.

I do not know whether you have yet noticed, perhaps as young men still being trained you have had little or no chance to notice it, that under common impulses men do get strangely alike. Probably there are officers here who have seen this. A single dominating emotion so prints itself on the faces of an organized body that the common differences almost disappear. You do not notice the individual members of a crowd if they are moved by any vast passion. What you notice is the passion itself, and it is often quite appalling. But such a passion, when it has been properly excited by the actions and words of a leader, will occasionally come back as a renewed stimulus with a tremendous force on that very leader himself. I do not know whether you remember a resounding phrase about a great traditional poet and the people for whom he sang. It was : " What they sent up to him in vapour he returned to them in flood."

May I then say once more that it is your work so to train your men and yourselves that, when the hour of stress comes, you will not only help them and lead them, but will also receive from them the unsuspected but mighty stimulation which comes from the undivided strength of a real organism devoted to one high end, the well-being and honour and glory of our country ?

CHAPTER XI

The Pharmakos and Medicine[1]

YEARS ago I began to wonder why the Greek scapegoat or outcast of the festival of the Thargelia was called a Pharmakos. I could not understand what connection there could be between the Greek words *φάρμακον* and *φαρμακεύω* and the scapegoat that many have called the Human Medicine. However, the matter passed out of my mind till I got a copy of the second edition of *The Rise of the Greek Epic*, and there Professor Murray's remarks in Appendix A brought the matter back to me. Professor Murray seemed to believe it was probably a foreign word, and, noting the long *α* in the Ionic, suggested that in Attic the *α* was short from analogy with *φάρμακον*. This seemed to imply that he regarded *Pharmakos*, the scapegoat, as differently derived from *φάρμακον*, the drug. Nevertheless, on page 34 of the *Greek Epic* he speaks of the *Pharmakos* as Human Medicine, which to my mind is a very late interpretation of the word. It certainly is a difficult problem to connect *Pharmakos* with a word for a drug or a man who used a drug, a pharmacist or physician. But following the clue which suggested a foreign origin, I sought for some other word in the same area which might suggest where it came from. I now believe that the original word

[1] *The Pharmakos*, "Folk-Lore," vol. xxvii. 2, pp. 218–224. Vide *Preface*.

and the two original roots which make it up came from the Turkic family of speech. For there is to be found in the Turkic tongues in various forms what looks like the very word itself. In Turkish itself it is spelt *vourmak*, which means "to beat." In this word *vour* is the root, which means "beat," and *mak* or, rather, *mag*, is the original root, both in the Turkic and Aryan families, which means "make." That *mak* is common to these two groups seems tolerably certain, though how it came to be in both nobody knows. We certainly cannot connect the Turkic with the Aryan group, and yet the root *mak* is very widely spread. Thus *vourmak* means literally "to make blows" or "to whip." It is odd that it is seldom employed in any Turkic tongue to mean beating with a stick or whip. In that case the root *dyon* is more commonly used. When we remember that in the Greek Ritual the *Pharmakos* was beaten with agnus castus, with squills and other flowers, that must have some significance. We may note that *vourmak*, "to beat," may just as often have the termination *mek* when the Turkish laws of euphony demand it. One of the Turkish substantival gerunds of *vourmak* is *vourour* or *vûrûr*, which seems to be, curiously enough, the exact philological equivalent of the Latin *verber*, a thong or whip, which is apparently an oddly reduplicated form. From this it seems the real meaning of *Pharmakos* is just a beaten or whipped person, and at last, by a later process of semantics, one who has been driven out with blows. Whether one is justified in bringing in Latin in this case is a matter of question, but it is certainly interesting to note that the reduplicated root in *verber* and *verberare* and in *verbero* (one who deserves a flogging) has in some ways a look as if it did not belong to the Latin tongue, but was an importation as in the Greek. It is certainly suggestive of the root *vour* or *phar.* I note in the old *Etymologicon* of Voss he says as regards *verbera,* "sed cum Salmasio dicamus *verber* esse ab æolico βερπύρ pro δερτύρ." Of course, no stress can be laid on this or on Voss. An interesting analogy is also to be found in the Greek *μαστιγίας,* a scoundrel.

According to this view, *φαρμακεύω,* "I give drugs or poisons," is, naturally, from the same roots. Its very existence implies an early medicine man, a Shaman, some one equivalent to those found with all their ritual among the Africans and Central Asians. Thus *φαρμακύειν* means, as it would with early races, "to drive out evil spirits with a whip, or with blows." Such a connotation is, on my theory, earlier than "to give poisons," but one knows that the ritual of the savage cure largely consists in driving out the spirit of disease or witchcraft by noisy incantations or by actual physical ill-usage of the patient. If I am right, it is curious to consider that our word "pharmacist" has for its early meaning exactly that of the ancient medicine man or exorcist.

There is another interesting point connected with *Pharmakos* which I have not seen mentioned. All over the East the word *farmaçion* is used with the meaning of an outlaw, and quite commonly with that of a cunning blood-drinking enemy of religion, a man who is a satanist or devil-worshipper. Of course, by a sort of meiosis it seems sometimes to mean a mere scoundrel, just as by a kind of hypokorisma the equally interesting word *epikouros* is used in Northern Africa, where this verbal descendant of the name of the great philosopher has come to mean an enemy of Islam, a Christian, and an atheist or a scoundrel. This is somewhat on a par with the use of the word " Atheists " for the Christians at the time of Julian the Apostate. There does not seem to me any doubt whatever that *farmaçion* is actually the same word as *Pharmakos.* It is used in Turkey and Asia Minor, and as far east as Afghanistan. It may be that the ancestors of the Greeks borrowed it originally from some Turkic race, and returned it again to the Mahommedans with a fuller connotation.

Oddly enough, the word *farmaçion* has, since its re-adoption by Eastern races, taken on a new meaning. It now often means " a freemason," one who is looked upon by the orthodox as an outcast and a scoundrel, a sufi and one highly irreligious. Not being a freemason myself, I know nothing of its ritual, but, so far as I can learn, members of this society, or those who are really instructed in its ritual and doctrines, regard their common name as one very uncertain in its etymology. Its present or common meaning is undoubtedly false philology. Our word freemason is, of course, a translation from the French franc-maçon, but to my mind " franc " is nothing but a metathesized form of the *vour* of *vourmak* and the *phar* of *Pharmakos* with an added euphonic nasal. Thus, it is only by a later verbal accident that the " maçon " was turned into " mason," and connected with masonry and building. Probably, then, it is actually the same root as the *mak* of *vourmak* or *farmaçion.* The early societies and secret orders of the East (the East, as might be expected, being full of secret orders) have linked themselves on to masonry as the last surviving order which used their secret marks. Probably, to begin with, these marks had no relation to building. It seems then that etymologically the freemasons are no more than a band of " pharmakoi."

To go back to the actual *Pharmakos,* one may note that Professor Murray is strongly of opinion that he was never killed, but only beaten. This is certainly borne out by my suggested etymology, although, of course, the very word Pharmakos may only have come into use when the ritual had been modified and humanized. It is interesting to note that there are two small islands off the coast of Attica, not far from Salamis and in the Bay of Eleusis, which were known in classical times as Pharmacussæ. On one of them used to be shown the Temple of Circe. There is another island on the coast of Asia Minor called Pharmacusa, where, according to Plutarch, Cæsar was taken prisoner by pirates when he was a young man. I cannot help thinking that in both cases these islands might practically be translated into English as Outcast Island or Islands. That is to say, they were originally refuges for wandering scoundrels, pirates, and the like, those who harried the settled mainland, and were looked upon as the Britons looked upon the Danes, and as the mainlanders looked upon some of the islanders at the time of the Migrations of which Professor Murray gives such a fine imaginative picture. There also is another island in the Bay of Iassus which is, I believe, still called Farmako. It is possible, of course, that such a name sprang from the fact that these islands were inhabited by survivors of the primitive tribes who were always apt to be looked upon as magicians.

Naturally enough, during the course of time there have been many attempts to discover the root meaning of *Pharmakos,* and I cannot help thinking that some of the later attempts are little better than those of the scholiast and grammarians. For instance, Eustathius derives *pharmakon* from φέρειν ἄχθος when used in a bad sense, and from *φέρειν ἄκος* when used in a good one. One does not always, even nowadays, get much help from those who ought to know. When my theory was submitted to one well-known Orientalist he said that the older or classical form of *vourmak* was *ourmak.* He was, of course, wrong. He was an authority on the Semitic languages, but evidently knew little of Turkish. It is impossible to speak of it as an old form when all existing Turkish documents, being in the Arabian character, must necessarily be subsequent to the eighth century, when the Turks of the Khanates were endowed simultaneously with Islam and the Persi-Arabic alphabet. Nor do I understand how he could have thought *ourmak* could have been degraded into the popular form *vourmak.* According to all philologic knowledge, any degradation would have been in the opposite direction. It may be noted that as there is no Arabic character to represent the *v* sound the Turks use the *wau* for this purpose. There are, in fact, hundreds of words in Turkish beginning with a *v* sound and thousands in which the *v* is incorporated. They are all represented by the Arabic *wau.*

In this paper I have not troubled to speak about the actual meaning of the *Pharmakos* ceremony. Professor Murray seems wedded to the belief that it was in every case a *mimema.* On the other hand, Sir James Frazer is equally certain that even in civilized Greece the Thargelian rites took darker forms than the mere expulsion of this quasi-religious outcast when he was beaten with agnus castus or squills and expelled from the city. Certainly, the derivation which I offer seems on the surface to support Professor Murray's contention. But the general body of anthropological lore on this subject points steadily to darker customs which may have been resurrected in classical Greece during the times of abnormal wrath on the part of the gods or in times of scarcity, if the *Pharmakos* represented, as he often must have done, the spirit of winter.

It would, of course, be interesting to get some early references to the use of *farmaçion*, but it is very difficult to trace any Oriental expression before mediæval times. One has to remember that using the pen was, in its way, a solemn rite. Up to the tenth century every sheet of writing was headed among the Mahommedans, " In the Name of Allah, the Compassionate and Most Merciful " ; and is still in all literary work. An Orientalist friend of mine to whom I have referred asks, " How, with such a headline, would a pious scribe dare to refer to a blood-drinking satanic *farmaçion* ? Such a combination might have made some dreadful formula capable of shooting the writer into the infinities of the *n*th dimension of space." Such an attitude of mind is especially characteristic of the Oriental. Although magic was utterly condemned by Mahomet, it was believed in none the less because he condemned it as a practice, and it is still believed in. My friend tells me that the word has been used for a long time in the traditional comments on a portion of the ritual of a secret society into which he was initiated in an obscure town on the Tigris. The actual early papyrus was totally indecipherable and belonged to no known language. Indeed, those who held these documents, which had probably been transcribed many times by men who did not understand the script, were of the romantic opinion that the original was to be referred to the era of Khamurabi, although the comments were probably not older than the eighth century A.D. Of course, such a statement as this is not evidence without further support. And yet, if the derivation of *Pharmakos* is what I have suggested, the use of the word certainly goes back beyond all historic times. Assuredly *farmaçion* must be a very ancient word, and the horror of the orthodox Islamite for it is natural enough. We may compare the Catholic Church and its views of Freemasonry. There were political reasons for this, but the Church has a deep-seated jealousy and dislike and even fear of secret societies.

While considering this subject I have come across some who actually declared that we might start the history of the word from *Odyssey* ix. 393. That is certainly of to-day compared with its real history, for even Hipponax of the sixth century B.C. had to explain it. And when this passage in the *Odyssey* uses φαρμάσσειν in the sense of to "temper," how is it possible for us to look on mere tempering as a primitive meaning when we know what we do of the whole body of Wayland Smith legends? A smith was always a magician in the old times. Of course, the scholiast interprets the word in this passage as "hardening." As a matter of fact, it was probably "curing." What a magic sorcerer or smith did was to cure the iron of its native softness and bewitch it, almost certainly with incantations and ritual, as he plunged it into the tempering medium. We might even say that he drove out the devil of softness. Wherever there is an element of magic in a word one expects that to be primary. The expression φαρμάσσειν χαλκόν, "to temper or strengthen brass," cannot be primary. One needs some imagination to deal with words like this. One of the weaknesses of the common dictionary is its habit of putting the usually accepted meaning first and the original meaning afterwards. So, when one looks at Liddell and Scott one sees φαρμάσσειν means, to begin with, "to medicate," and secondly, "to enchant or bewitch by the use of potions." The word certainly goes back to the ages of magic ritual, and back again to the very expulsion of Jonahs, people who had no luck and brought ill luck, probably before magic itself was practised. It is a natural animal instinct to turn out those who seem to bring ill fortune, even if there is no piacular element in such an expulsion. Animals often expel some of their kind. We may compare rooks and elephants and even cattle, who kill a wounded member of the herd when his loud lowing might possibly bring them into danger.

Of course, it is exceedingly hard to say, when we consider what a linguistic whirlpool Asia Minor has always been, what was the actual origin of this particular word. It might not originally be Turkic. There is a strange tendency among certain people to attribute everything unknown to the Hittites, but, as no one seems to know what Hittite is, that is very little use to the investigator. *Vourmak* may not, of course, be Turkic at all, although it is a living word in the living Turkish language at the present time.

APPENDIX A

THE INFECTION THEORY OF CANCER

IT was obviously impossible to discuss in the text all the theories of cancer in detail, but as I understand that some think the paper less than just to the case presented by those who support the infection theory, something more may be said of it. It was, perhaps, too much to infer without a more special examination that the parasitic hypothesis of malignancy is only explaining one mystery by another. The real objection to this hypothesis is that even if it were found to be a fact that an infection precedes all malignant growth, it would remain a mere observation and not an explanation. What we want to know is *why* invasive cell-proliferation takes place, and whether excessive cell-growth is peculiar to such diseases, or can be found in others. Any bacteriological explanation of malignancy must classify the phenomena among those which are recognized as being symptomatic of infection. So far this has not been done, and a provisional assumption that the imagined infective agent is so different from all others as to be able to produce the phenomena in question exhibits many of the marks of vitalism, *i.e.* it invests the agent with the unexplained power of producing what we desire to explain. Logically an unknown entity capable of causing the effects in question must not be propounded as a cause if any evidence can be brought to show that known causes can produce them. If so they can be classed. If they are unique phenomena in infection they obviously cannot be classed. But as overgrowth is not a unique phenomenon among developmental diseases, and as we know that it can be caused, or inhibited, by chemical cell-products, we are entitled to declare that infection, if a cause, is a secondary one, and that the real cause lies among the phenomena of growth as much as acromegaly and other disorders due to unregulated stimulation of organic tissues.

It is impossible to argue clearly upon so complex a subject unless the nature of explanation is really understood. We need but ask any one to "explain" explanation to find how few have clear ideas on the subject. According to the logicians a partial comparison is not explanation. The phenomena of one science are only to be regarded as explained when they can be classed among the observed sequences, or so-called laws, of a more inclusive science. Therefore the pathological phenomena of malignancy must be capable of classification with regard to physiological phenomena as we must regard pathological facts as deviations from the average or normal tissue. In the paper I endeavoured to range the facts under physiological and biological laws, since biology includes physiology and pathology. However unsuccessful the attempt may have been, it is obvious that it was not made without reason or without results, whereas the attempt to range them under the observed sequences of bacteriology fails in vital particulars, and leaves us just where we were, that is, in presence of a possible infective agent which is hypothetically given the qualities that produce the effects with none of the known signs of infectivity. For until a malignant growth becomes really infected it is, so far as its cells are concerned, obviously in riotous health, and although symbiotic alliance occurs in lower organisms such as *Convoluta roscoffenis* the symbiosis is not intracellular, as the infective, microbic, or protozoal theory of cancer demands.

Moreover, even if a special infection hypothesis were proved to be correct, we should still have to seek the cause of the tissues acting as they did when affected by, or in symbiosis with, the pathogenic agent. Till that is understood we are still in the dark, and the particular organism which causes disaster remains no more than a secondary or exciting cause. It is, therefore, strictly logical to range all infection causes as irritative, or irritative and weakening, agents, whether they be one or many. Lambert Lack teaches that periodontitis is a common cause of tongue cancer, by which he means that its toxic products acting in combination with other irritation and syphilis in some way urge the epithelium into revolt. Such a view is logically sound. It is stated by Peyton Rous that fowl sarcoma can be reproduced by the injection of a filtered extract of the original malignant growth. It is therefore inferred by some authorities that the reproduced disease is due to a filter-passing organism. So long, however, as such a filter-passer cannot be cultivated, or shown to exist by other methods, it is only legitimate to infer that such a phenomenon must be ranged among those caused by irritative agents or toxins pulling the trigger of an unstable tissue. Moreover, even if the suspected filter-passer is proved to exist, the only logical inference to be drawn is that an infective organism can excite sarcomatous overgrowth in the fowl's tissue just as other irritative agents can produce carcinoma. The evidence takes us no further than irritation. Even if it were proved that a sterilized injection of this sarcomatous growth failed to reproduce the original disease, it would still be far from proved that more than irritation was needed to cause it. It is, indeed, quite probable that the toxins of the injection, if not broken up by the process of sterilization, might still produce the sarcoma by abnormal stimulation of the connective tissues, a view in accordance with those suggested in the paper. If malignancy were caused by the suggested agents we should expect like results to be found in more cases than this. I suggest that such experiments in aged fowls might produce carcinoma, not sarcoma.

Certain results obtained by Bashford with regard to the immunization of mice to mouse-cancer by the injection of mouse skin, need further elucidation. I am not sure whether pure epithelial products were injected, or whether connective tissue was used with it. According to the developmental theory it may be suggested that if epithelium was used alone, mouse-cancer might more properly be called a sarcoma. But in any case it remains very suggestive that animal tissues, whose action must be in the nature of the products of the endocrine organs, do produce inhibitory effects. It is, perhaps, the more surprising that the work of Shattock, Seligmann, and Dudgeon, when they attempted to produce chondromatous growths by grafting fœtal bones, did not lead them in the direction of the developmental view rather than in that of parasitism or infection. Their statements as to restraining bodies, or " corpora cohibentia " are strictly parallel with the doctrine of the endocrines which obviously inhibit as well as stimulate. They state that " the cartilage of the body (like each of the other tissues) tends, we may assume, *per se*, to grow indefinitely and without limitation. But against this inherent endeavour, each of the other tissues, other than the cartilage, furnishes a restraining substance, and co-ordinates its growth with the rest." If this is true, and few will nowadays be found to doubt that it represents, at any rate partially, the actual machinery through which ordered growth is obtained, it follows inevitably that the want of a particular restraining body should result in the disorderly growth of some tissue specially apt to proliferate. Such are epithelium and connective tissue, the agents of malignancy, and in view of the whole of the phenomena of malignant disaster and repair it is perfectly legitimate to infer that these two tissues are those which react most powerfully upon each other.

Other experiments by Shattock and Dudgeon are of great interest and value, and at first sight may seem to support the parasitic theory. Mice fed with mouse-cancer appear to have developed, (1) a round-celled sarcoma, (2) an invasive epithelioma of the mediastinal glands, (3) an invasive endothelioma. Such results are, however, by no means convincing. Abnormal results from highly abnormal foods are what might be expected, on the general theory of physiological balance. Salmon and trout fry fed upon pig's liver develop a thyroidal overgrowth which later may become malignant, and the ingestion of foods, carrying stimulating or inhibiting products or both, should on the developmental view have abnormal results. It has been said that excessive thyroidal medication is in some cases followed by cancer. Moreover, the statement by the two workers above mentioned really supports the developmental view. They say "the first striking thing is that the tumours of the three mice have not the same histological character, and that none can be viewed as having resulted from the growth of implanted cells." If, however, such tumours resulted from a definite parasite, we should look for a repetition of the original histological structure. From the point of view of endocrine action, however, nothing is more likely than that the subjects of the experiment should break down in different ways. One mouse cannot have the exact resistance of another, nor could we look for the same results in all of them any more than we should expect soot carcinoma in every chimney-sweep.

It must be insisted on that the developmental theory of cancer is as definitely against cancer propagation by transplantation of cells from one patient to another as it is against a definite cancer parasite. When Shattock endorses Paget's view that malignancies in families is not due to such causes it is impossible not to agree. But such an agreement by no means reduces us to the necessity of admitting infection. A family with inherited tissue instability, if exposed to conditions likely to decrease health and further impair tissue balance, may experience any form of malignant growth. Quite independent of such diseases families can be found in which one member is hyperthyroidal, another an athyroidal dwarf, and another myxœdematous. That families subject to malignant disease die of various forms of it is distinctly against parasitism.

It is argued that some of the phenomena of radium and X-ray malignancy imply a second factor, probably, or at least possibly, a parasite. But in X-ray cancer nothing more appears to be needed than the disturbing effects of the rays themselves. It is difficult to suppose that any organism is always infected with an organism ready to display its activities on X-ray excitation ; a supposition which is necessary as such rays, if sufficiently applied, always produce malignancy. With regard to radium, I understand that Lazarus-Barlow claims to have produced by it a "pre-cancerous" growth in rats and rabbits. These altered tissues were only slightly invasive, and when the radium action was stopped they resumed their normal character. It is held by some that this proves the necessity of a second factor which may be parasitic, but the strictly logical view is simply that the organism recovered, *i.e.* that the normal tissue inhibitions were restored, and that the altered tissues resumed their ancient functions. Since we know that, even in obvious cancer, attempts of the organism to cure it succeed for long periods, it is illegitimate to infer that an artificially produced morbid state of possible malignancy in a healthy subject may not revert to a normal condition when the disturbing influence is removed.

With regard to the difference between benign and malignant tumours, which some think are such as to divide them entirely into classes of wholly different causation, it may be said that much late work does not support this view. The fact that so many benign growths at last become malignant, and that in others the dividing line is so obscure that the histologist and pathologist are doubtful as to their character, obviously suggests that a further want of inhibition or the increase of some undue stimulation may end in malignancy. A few authorities argue that even benign tumours are parasitic in their origin. All that is necessary to say of this is that such a theory of causation appears superfluous.

Perhaps the best defence of the parasitic theory is that of D'Este Emery (*Tumours*, 1916). It is well argued, and likely to convince those who are already inclined to take the view supported. Many of the arguments, however, will not bear critical examination. Emery and C. P. White seem to think that a continuously progressing proliferation of malignant tissue demands a continuously increasing irritant. This is surely fallacious. If there is any loss of balance in a body or any other structure, less and less power is needed to cause destruction. The Leaning Tower of Pisa should take less to overthrow it now than when it was built. If the theory of organic restraint has any basis at all, and no one can deny that it is very firmly based, any such disaster as malignancy may be compared to a breach made in a dam. To remove the highest part will take much labour, but as water begins to flow potential energy becomes kinetic, and the whole dam goes. In malignancy we need not posit an increasing power, for what we see in most cases is a decreasing resistance, as every pathologist recognizes that there *is* attempted repair in cancer, even though it mostly fails. Emery also says, "if growth is very rapid, the innermost layer of the epithelium *may* find it easier to grow downward into the tissues than up." The italics are mine. The developmental or hostile symbiotic view explains such a fact. Indeed it shows that the necessary preliminary to such downward growth is a failure of the connective tissues which is plainly demonstrated by the phenomena accompanying excessive radiation. With irritated reacting epithelium it is not "may" but "must."

According to D'Este Emery malignant cells frequently act as phagocytes, which suggests they have some power of movement. This is surely a forced interpretation of the destructive nature of malignant cells. That they erode tissue, and even "eat away bone" (Bland-Sutton) is true enough. But we do not call the erosive action of the chorionic trophoblast phagocytic, and, as is said in my paper, Handley states that the multinuclear cell of cancer appears when met with resistance such as the blood possesses. That the cells "move" is true, but their movement, which can be measured, is the thrust of growth, and room is obtained for that by the cell catalyst and mechanical pressure.

Emery's views upon X-ray phenomena are to be found in less than a dozen lines. He remarks that the cracks seen in X-ray dermatitis are portals of infection. Such infections as entert hat way are, however, those which can be proved to be known agents, and the theory ignores the facts that long before cancer is recognizable there is, underlying the irritated epithelium, the true pre-cancerous stage of rarified connective-tissue elements. Would the author affirm that continually sterilized hands exposed for long periods to X-rays would not become malignant?

The argument based upon cage infection does not carry much weight. Captive animals are already in an unnatural condition. It is notorious that their general powers of resistance, and especially their powers of repair, are weakened. If many of them break down with malignant disease, what is the most that can be logically inferred? It is that in bad conditions and poor health some infection, if it is an infection, may cause irritation and malignancy. But no one ever denied that infections can be irritative causes of epithelial overgrowth.

Another argument is that malignant tissue growing alongside normal epithelium shows that the latter has a power of resistance. It is difficult to see what is inferred from this. It is to be expected from all the phenomena that to begin with there should be a local breakdown of balance at the point of irritation, and the spread of it into normal epithelium which does not immediately revolt is no more surprising than that a social riot, in which the police have been overpowered, should spread, and yet be repelled for a time in areas where they are still strong, and the inhabitants have not been excited to disorder. It is no vain metaphor to suggest that in the body politic the police greatly resemble the wandering connective-tissue cells of any organism.

In conclusion, it may be repeated, and even insisted on, that only the developmental and endocrine theory shows any reason whatsoever for the basal facts of malignancy, that is, for the actual undue proliferation of tissue cells, their tendency to revert to an embryonic character, and their power to spread beyond normal boundaries, while the biological conception of the organism as a federation of cell-colonies working in a harmony, which is the result of "constraining bodies," throws a brilliant light on invasiveness as the result of a failure in such inhibitions.

REFERENCES

EMERY D'ESTE.—"Tumours," 1916.

SHATTOCK and DUDGEON.—"Feeding Experiments with Mouse-cancer," *Proc. Royal Soc. Med.*, vol. x. p. 35, 1916–17.

APPENDIX B

THE *PERONEUS TERTIUS*

IT seems as if the *peroneus tertius,* a muscle only found in the human organism as a special portion of the mechanism for preserving the structure of the foot from damage in orthogrades, is a key case for the opponents of transmission. This muscle is a lesser opponent of the *tibialis anticus,* which is found well developed in simians not using, or only rarely using, the upright posture. It must, then, have come into existence at a late period of evolution. This is emphasized by the fact, that a few fibres of the *tertius* are occasionally found in the gorilla. Is such a direct adaptation to be attributed to germinal variations, that is to say, to accident, if we hold that the germ-plasm does not respond magically to new needs? To me this seems much more than unlikely. A new muscle has arisen at a special point of strain as a part of the set of muscles preserving the arch of the foot and, as we see from pathology, it is not yet doing its work with complete success. It is still imperfect. If such a muscle is not a direct adaptation to new stresses words have ceased to have any meaning. To say that a single muscle cell or fibre arose from a "spontaneous" germinal variation, and was found advantageous, is to make a mockery of mechanism. But if it is assumed that continual stress can produce such a muscle reaction we may infer that continued strain would reinforce it. We can, in fact, prophesy that in time such continued stresses will secure adequate response. We must also infer that, when a set of muscles is used in a new position, as was the case when the upright posture was adopted, opposing muscles are no longer properly counterbalanced. Some new muscle is called for. It is obvious that such new strain must be repeated in embryo if the muscle is to increase, since new muscles fibres do not come into existence later. As the embryonic *tibialis anticus* begins to exert a pull, these stresses are felt by yet undifferentiated cells, the parents of muscle cells, which are capable of becoming muscle if so stressed. Without such embryonic strains the tissues would have altered in some other manner. If language is not to be tortured into something which serves no function, we must come to the conclusion that new stresses are repeated during development, and exert a morphogenetic influence upon the undifferentiated tissues. If this is so transmission is no longer guesswork.

APPENDIX C

Marcus Terentius Varro

As this passage seems little known I have transcribed it here, although it is an uncertain, and perhaps corrupt, text.

De Re Rustica. Book i., chap. xii. "Sin cogare secundum flumen ædificare, curandum ne adversam eam (villam) ponas: hieme enim fiet vehementer frigida et æstate non salubris. Animadvertum etiam, siqua erunt loca palustria, et propter easdem caussas, et quod (arescunt) crescunt animalia quædam minuta, quæ non possunt oculi consequi, et per aera intus in corpus per os et nares perveniunt atque efficiunt difficiles morbos."

Fundanius, "Quid potero" inquit, "facere si istius modi mi fundus hereditati obvenerit, quominus pestilentia noceat?"

"Istuc vel ego possum respondere," inquit Agrius: "vendas quot assitus possis, aut, si nequeas, relinquas. . . . Prætera quod a sole toto die illustratur, salubrior est et bestiolæ siquæ prope nascuntur et inferuntur, aut efflantur aut aritudine cito pereunt."

"But if you must build beside a river you must take care that you do not put the house to face it: for in winter it will be excessively cold, and in summer unhealthy. For the same reasons you have to be on your guard against marshy places, and also because (as they dry) minute animals are engendered there which cannot be detected by the eyes, and these borne by the air get into the body through mouth and nostrils, and cause diseases difficult to get rid of."

Fundanius said: "What should I do to avoid the evil of infection if I were to inherit an estate of that kind?"

"I can tell you that," replied Agrius. "Sell it for what you can get, and if you can't sell, leave it. . . . Besides, the house is healthier for being shone upon all day, and if any animalcules breed and are carried there, they are either blown away or quickly perish through the dryness (of the air)."

INDEX

PRINTED BY
MORRISON AND GIBB LIMITED
EDINBURGH